THE JOURNAL
OF THE
ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

No. 186. JUNE, 1926. Vol. XXX.

NOTICES

Elections

The following members were elected at a Council Meeting held on May 16th:

Associate Fellows.—Mr. J. H. Crowe and Mr. D. A. Hughes.

Associates.—Flying Officer E. St. M. Brett, Mr. O. P. Denyer, Flying Officer C. A. C. Fidler, Mr. C. H. Frewer, Mr. W. J. Plater, and Lieut. The Hon. J. M. Southwell, R.N.

Students.—Mr. E. J. Fearn and Mr. P. T. Capon.

Members.—Mr. E. A. Berens, Mr. N. Norman, and Mr. R. C. Tragett.

Associate Fellowship Examination

Provided that sufficient entries are received, the Society's examination for Associate Fellowship will be held on Monday, September 20th (Part I.) and Tuesday, September 21st (Part II.). Entry forms may be obtained from the Secretary and must be returned duly completed not later than Monday, August 23rd.

Erratum

The name of the author of the paper on "Aerofoils" which was published in the May issue is A. S. Sinnatt, M.C., D.Sc., M.Sc., and not as given in that issue.

Council Dinners

On April 19th, 1926, a dinner was given at the Royal Air Force Club by the members of the Council to Mr. Harry Guggenheim and Admiral Cone, on the occasion of their visit to Europe in connection with the administration of the Guggenheim Fund.

On May 27th, Mr. F. W. Lanchester was the guest of honour at a Council dinner given at the Royal Aero Club after his lecture on "Sustentation in Flight" before the Society. At the lecture Mr. F. W. Lanchester was presented with the Gold Medal of the Society for his researches and pioneer work in aeronautics.

Endowment Fund

The Council desire to record their grateful thanks to Shell-Mex Limited and the British Petroleum Company Limited for their generous donations of

£50 each to the Endowment Fund. The following is the list of donations to this Fund to date :—

	£
Sir Charles Wakefield	250
"A Friend of Aviation"	100
The Anglo-American Oil Company	50
Mr. H. Scott Paine	50
Shell-Mex Limited	50
British Petroleum Company Limited	50
	£550

It will be remembered that Lord Cowdray offered to give £50 a year for five years if nine others would do the same. A member has promised to give the final fifty pounds a year when eight others have promised their contributions.

Donations

The Council desire to record the following gifts which have been made to the Society during the past month :—A set of photographs relating to Bleriot's first flight in a monoplane, given by Lieut.-Colonel F. R. Durham; a quantity of back numbers of the Journal, Aeronautical Society Reports, newspaper cuttings and several books, given by Colonel J. D. Fullerton; a photograph and pamphlet relating to the work of F. J. Stringfellow, given by Mrs. Green; and a parcel of bound copies of *Flight*, from Lieut.-Colonel M. O'Gorman.

Students' Section

The visit to the Fairey Aviation Works was cancelled owing to the strike.

The visit to the Royal Aircraft Establishment, which was postponed owing to the general strike, will be held on Wednesday, June 16th, unless the present coal stoppage interferes with the work going on there. Meet at Waterloo Main Line Booking Office at 10.0 a.m. unless further notice is given. Members wishing to join the party should send in their names at once to the Secretary, stating their nationality.

List of Members

Members may have copies of the stencilled List of Members on application to the Secretary.

Prizes

The regulations for the Edward Busk Memorial Prize of twenty guineas, and for the R.38 Memorial Prize of twenty-five guineas may be had on application to the Secretary. Entries for the former must be received by September 30th and for the latter December 31st.

J. LAURENCE PRITCHARD, *Hon. Secretary.*

PROCEEDINGS

EIGHTH MEETING, FIRST HALF, 61ST SESSION

A meeting of the Royal Aeronautical Society was held in the Society's library, at 7, Albemarle Street, London, on Thursday, December 4th, 1925, when a paper on "The Control of Stalled Aeroplanes" was read by Professor B. Melvill Jones, A.F.C., Associate Fellow. Air Vice-Marshal Sir Sefton Brancker, K.C.B., A.F.C., Fellow (Chairman of the Society), presided.

In introducing the lecturer, Sir SEFTON BRANCKER said: I hardly need introduce the lecturer to you to-night. He is so well known to you all from his long connection with aviation. He was at the National Physical Laboratory for three years before the war. When the war began he joined the Royal Aircraft Establishment, and during the war he was one of those scientists who put their theories into practice, became a pilot, and actually flew on the front. After the war he remained in aviation, and, as you all know, he now is the Mond Professor of Aeronautics at Cambridge. He is going to speak on a subject which in my opinion is of vital importance to civil aviation.

THE CONTROL OF STALLED AEROPLANES

BY PROFESSOR B. MELVILL JONES, A.F.C., ASSOCIATE FELLOW.

I must begin my lecture by explaining why and in what capacity I am giving it. As an independent member of the Air Research Committee of this country, I have been serving for some years upon a panel appointed by it to co-ordinate research upon the control and stability of aeroplanes; I have therefore been in close touch with the efforts which have been made throughout the country to improve the pilot's power of controlling his craft, particularly in that mode of flight known as stalled flight, in which the control of most aeroplanes of the present day is seriously defective. These researches have at length reached a stage at which it has been deemed desirable to prepare and issue a comprehensive report dealing with the course which they have taken and the results which have been achieved. Such a report has been prepared and has gone to press,* and I presume that your Council, in asking me to lecture upon this subject, desire me to deal with the subject matter of this report in a manner rather more brief and breezy than is fitting for a government report on a technical subject. This, in any event, is what I shall try to do, having obtained the permission of the Research Committee to use the information in this report, should it not have been issued at the time of delivering the lecture. You will understand, however, that I am in no sense lecturing in any official capacity; the observations which I shall make and views which I may express are purely private expressions of opinion.

The report to which I have referred treats the matter very thoroughly; certain sections deal with the broader aspects of the problem in a manner suited to the general reader, whilst other sections, intended more for the information of the specialist, enter into greater detail. There is therefore no necessity for me to attempt a scientific exposition of the problem, and for this relief both you and I may be thankful; it will, I think, be fitting to concentrate attention to-day upon the outstanding results of the research and their probable reaction upon practical aeronautics.

* Reports and Memoranda of the Aeronautical Research Committee No. 1,000.

It is just over two years since last I addressed you upon this subject. I find on looking back at my lecture* that I expended some space in emphasising the importance of obtaining adequate control in the stalled condition. To-night I think such a use of space is unnecessary; if I am able to judge the trend of public feeling rightly, the importance of this matter is now thoroughly realised. I will only say that the urgency of achieving, for all aircraft, really satisfactory control in the stalled state, is no less now than it was then. The bulk of the aeroplanes flying to-day are not yet satisfactory in this respect, and now that means are known for making them satisfactory it is to be hoped that steps will be taken to make them so with the least possible delay. We have the Croydon accident fresh in our mind, when a forced landing in difficult circumstances was rendered fatal to all concerned by just such a loss of control as we are discussing. Anyone who is, as I am, in touch with all the accidents which happen in the country, needs no other reminder of the vital necessity of ensuring that the incipient spin, which is the consequence of loss of lateral control in the stalled state, should no longer be enabled to catch the pilot off his guard, just when he is faced with the most difficult moments in his flying career—the moments immediately preceding a forced landing.

One other observation I wish to make before coming to the subject matter of my lecture. I still hold firmly to the belief that improvement of control after stalling is not alone sufficient to make the carrying of passengers by air a reasonably safe proceeding, unless it is accompanied by some effective limitation of the magnitude of the stalling speed itself. If improvement in control in this respect is immediately to be used as a reason for allowing stalling speeds to rise, the situation will be little, if at all, improved, and I do desire to state, as emphatically as I am able, the opinion that the mastery of the problem of stalled control in no wise affects the necessity for limiting, by regulation, the stalling speed of aircraft carrying passengers for hire.

It is interesting to compare the state of knowledge now with its state when I lectured to you in 1923. We may ask ourselves the question—has any progress been made since that date? The answer to this question will be yes or no, according to our view of progress. If we think of progress as ceasing after the nature of the solution to the problem has been suspected, the answer is No; little or nothing is known now which was not suspected then. If, however, we measure progress by the extent to which knowledge can be applied to practical life, then the answer is Yes; very great progress has been made. When I lectured to you before, I was unable to draw any clear-cut conclusions from the work then in progress; for our information at that time, though suggestive, was insufficient to allow this to be done with confidence. Now the situation is different; the initial stages of the motions which follow from a wide variety of disturbances from steady stalled flight have been followed by detailed calculation, and all the conclusions to which I shall refer have been checked by actual trial in free flight. It is therefore possible to present my lecture to-day in a much simpler form than before; not because further study has shown the problem to be less complicated, but because, being more sure of my ground, I am able to confine attention to aspects of primary importance, with some confidence that the simplified generalisations which I shall employ actually represent the true facts.

May I be allowed to digress from the strict letter of my subject and to make a few semi-philosophical observations on the relation between theory and practice. Whatever the layman or the non-technical Press may think, successful designers of machines and structures do not begin by covering sheets of paper with calculations involving x and y and the integral sign; they take the back of an envelope and a stub of pencil and sketch and plan in a very simple manner, calling upon their past experience and checking their ideas against a

* *Journal of the R.Ae.S.*, October, 1923.

number of well tried "rules of thumb." So used are they, indeed, to this process that they sometimes forget that the very rules of thumb which they use so easily, may be the condensed result of difficult and laborious researches and calculation. Therein I believe lies one of the root causes of the everlasting war which wages between the more ignorant designers and constructors and the more ignorant scientists. The former believes in his heart that all the rules of thumb which were in existence when he started to learn his trade were gifts from heaven, or the boiled down quintessence of a mysterious faculty called common sense, which he arrogates to himself alone. The latter considers that his semi-developed theories, based probably upon insufficient or faulty premises, should be applied immediately to practice, and he rails at the futility of the practical man who does not immediately apply them.* It may be said then that the test of whether a research has reached a stage at which it becomes of real practical value lies in the possibility of reducing its conclusions to simple "rules of thumb," capable of direct application to practical design. I have held this view about the relation of theory and practice for many years and I still hold it, so that I have found it interesting to test the state of the research under discussion, by attempting to define its main results in the form of two statements, which are sufficiently simple and general to be elevated to the exalted rank of "rules of thumb." Here they are:—

To cause an aeroplane to be thoroughly controllable when stalled it is necessary:—

(1) To provide ailerons which can exert a sufficient moment about an axis passing through the centre of gravity of the aeroplane inclined through a sufficient angle downwards and forwards from the mean chord of the wings.

(2) To provide a rudder of sufficient power.

Compliance with either of these rules results in an aeroplane which can be kept fairly well upon an even keel when stalled; one in which the deadly incipient spin need never be allowed to occur. Compliance with both rules is however necessary to obtain a good positive lateral control under all conditions.

Much turns upon the amplification of the word sufficient, on the three occasions in which it occurs in these rules. It is obviously impossible to lay down exact figures as to what will give sufficient control, until that blissful day arrives when all pilots agree as to what sufficient control is. But it is possible to give some indication as to what will lead to obviously satisfactory or unsatisfactory conditions.

Conventional ailerons on a stalled wing exert their moment about an axis inclined *upwards* and forwards through angles between 15° and 30° from the wing chord, the maximum moment which they can exert being represented by a coefficient † of about 20. This control is practically useless. At the other extreme, an arrangement, to which I will refer in more detail later, consisting of one of Mr. Handley Page's leading edge slots combined with a balanced aileron of the type known as "Bristol-Frise," will produce a moment for which the coefficient is 35, about an axis inclined *downwards* and forwards through

* In making these remarks I am, of course, safe from reprisals; for I insult no single individual, because no individual will ever admit that he belongs to an ignorant class, either of designers or scientists.

† This coefficient may be looked upon as a number defining the relative moment exerted by different controls, after allowance has been made for variations in air density, size and velocity of the aircraft. Its precise definition is $1{,}000 \times \text{Moment}/\rho V^2 S s$, where

ρ is the air density,
V the velocity of the aeroplane,
S the area of the wings,
s the semi-span of the wings.

The same coefficient will be used later to express rudder power.

some 10° to 15° from the wing chord; when applied to its extreme limit this device will produce a moment of coefficient 60, about an axis roughly parallel to the wing chord. This device, tested in free flight, has proved most satisfactory; it allowed the pilot to roll a completely stalled aeroplane rapidly from side to side, to right it after stalling upon a steep turn, and instantly to stop a full spin; all without using the rudder, and often with the rudder set actually against the recovery. Here then are extreme instances of devices which are, on the one hand, entirely unsatisfactory and on the other hand entirely satisfactory. Time and experience alone can decide whether some intermediate arrangement, giving less moment about an axis less inclined downwards, would be sufficient for practical purposes.

To interpret the meaning of "sufficient rudder power" we must look a little more closely into the reason why a powerful rudder is required in the stalled state. The first point to notice is that it is not required so much to cause the aeroplane to yaw, as to prevent it from yawing in an undesirable direction. An essential feature of the incipient spin is that it consists of a combined roll and yaw, in the sense that the falling wing tip is retarded; if this yawing motion can be checked, or reversed, by the use of the rudder, the incipient spin can be stopped.

Now, when the ailerons are not used, the yawing moment which the rudder has to counteract arises mainly from the fact that the wings are rolling, and it is easy to see how this yawing moment is generated and that it will be definitely limited in magnitude. When a stalled wing is rapidly rolled the rising tip meets the air at a smaller incidence and becomes unstalled, whilst the falling tip meets the air at a greater incidence and remains stalled. Now the air reaction on an unstalled wing acts forward of the perpendicular to the chord, whilst that on a stalled wing acts almost perpendicularly to the chord. Hence the rising wing is forced forward more than the falling wing and a yawing moment is called into play. But there is a very definite limit to the amount by which the reaction upon a wing can be inclined forward of the perpendicular to the chord, so that there must be a corresponding definite limit to the yawing moment generated by a rate of roll, no matter how fast the rate of roll may be.

Wind tunnel experiments upon the effect of rolling show this to be so, and no wing yet tested has shown a coefficient of yawing moment much greater than 10, whilst for normal thin wings the maximum coefficient is of the order of 8 or 9. It does not require a very rapid rate of roll to generate a yawing moment of this magnitude, but no further increase in the rate of roll generates a yawing moment greater than this.

It follows that rudders which will produce, on stalled aeroplanes of the types tested, a yawing moment of coefficient greater than 10, will be able to deal with the moments generated by the wings. When they can do this, theory and experiment agree in confirming that they can always be used to restore the aeroplane to an even keel, by forcing forward the falling wing and so generating a rolling moment in the sense to lift it again.

Fig. 1 gives some idea of the powers of the rudders of various aeroplanes. In this figure abscissæ are the incidences of the main wings and ordinates the coefficients of the yawing moments given by the rudders moved through 20° from their neutral position. Several of these rudders could be moved further than this, with a consequent increase in moment, but 20° has been chosen for comparative purposes, because experimental wind tunnel data existed for models of all these aeroplanes with this rudder setting.

The universal falling off in rudder power on stalling should be noticed; this is due to the rudder becoming shielded from the relative wind by the body and elevators. It will be noticed that this reduction of power, considered as a

proportion of the power in normal flight, is, as might be expected, much greater for small weak rudders than for large powerful ones. The interesting region from the point of view of the present discussion is that between 20° and 30° incidence of the main wings, and here the effective rudder power of most of the aeroplanes tested is much less than the coefficient of 10 which was considered the minimum desirable a few moments ago. The only standard aeroplane which approaches these requirements is the S.E. 5. Here the rudder could be moved through over 30°, and the maximum moment developed had a coefficient greater than 10. Unfortunately I have not been able to obtain reliable statistics as to the number of accidents which have occurred to this aeroplane through the incipient spin;* but in my own experience I know of none. Perhaps some one

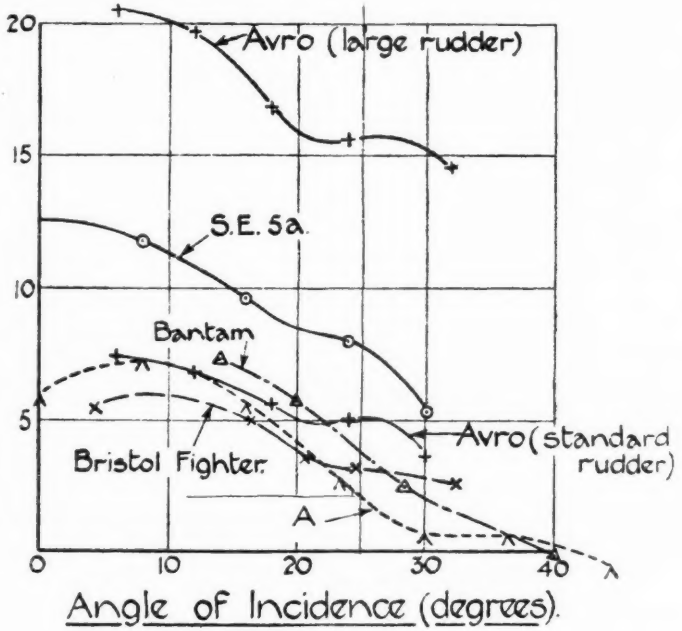

FIG. 1

in the audience may be able to provide information on this question. The large-ruddered Avro was a purely experimental aeroplane with a fantastically large rudder, which has in fact been found to be larger than is required to check a spin in any stage of development.

Turning to the other end of the scale we see, in model A, an example of rudder shielding, so great at very high incidences, as to render the rudder practically useless. This model was used in an interesting research upon shielding of rudders, and Fig. 2, taken from R.M. 965, shows some results of this research, which will help me to describe the features in design which lead to defective rudder power in stalled flight.

A is an elevation of the tail of the model in its original form. Here the

* Accidents due to a long continued spin have no bearing on the present discussion, the processes involved in checking an established spin being different from those for checking an incipient spin.

fin and rudder are low and do not project beyond the body, which was oval in section. This arrangement gave the defective moment curve A which we have already considered and which is shown above.

B shows the rudder and fin greatly increased in area and moved backwards,

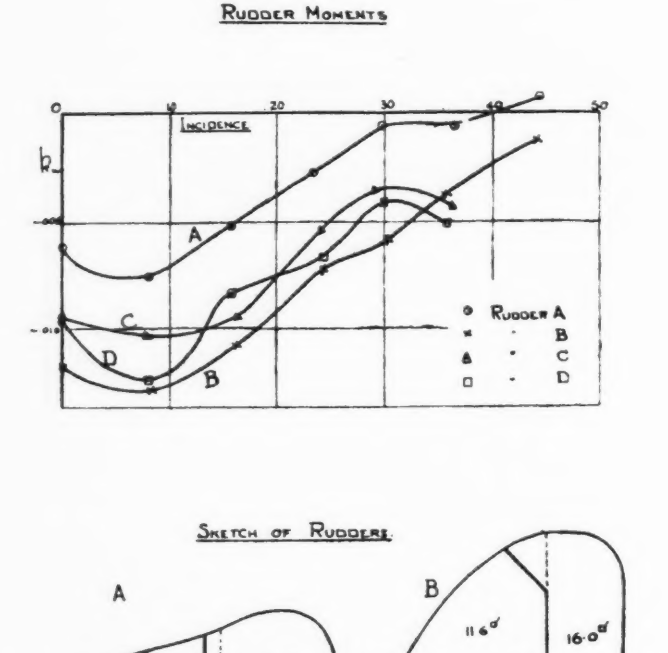

FIG. 2.

so that the rudder projects behind the body and works in a Vee-slot in the elevators. The rear part of the body is also altered so as to end in a vertical knife edge, and is raised so as to make the top line of the body horizontal in flying position. This arrangement gave the greatly improved moment curve shown above.

C and D are alternative arrangements, giving nearly as good results as B, but with less fin and rudder area. In C the effect is obtained by raising the rudder, and in D by moving it backwards and causing it to project well beneath the body.

In general it may be said that a low fin and rudder, which does not project behind the body, or is directly over a continuous elevator, will be defective when the aeroplane is stalled, and that the defect will be accentuated if the body is short and thick, broad in plan near the tail, and has its upper line much curved downward.

It is not always practicable for the designer to have the rudder power of his aeroplane tested out on a model in a wind tunnel, and he will therefore want to know how large to make his rudder in order to give the required power. No exact figure can of course be given, because exact figures depend, as we have seen, upon details of design. When the rudder is small and badly placed in relation to the body, its power may be very dependent upon these details of design, but if it is on the large side by comparison with present practice and attention is given to the points mentioned, its power is not so sensitive to details of design and can be roughly estimated from its size and position.

The power of the rudder depends upon its own area and that of the fin in front of it, and upon the distance of those areas behind the centre of gravity of the aeroplance. A convenient way of comparing these quantities in different aeroplanes is to multiply the combined area of fin and rudder by the distance of their centre of area behind the centre of gravity of the aeroplane, and to divide this product by the area of the main wings and their semi-span. The resulting ratio may be written $S''l''/Ss$ and is often called the "rudder volume," because the dimensions of each half of the ratio are those of volume. In the majority of aeroplanes of the present day l'' is not far different from s, so that the "rudder volume" is approximately the ratio of rudder and fin area to wing area. This rudder volume cannot be a complete criterion of the action of the rudder, because it does not take account of the proportion of area of rudder and fin, but if the rudder is not appreciably less than half the total, it is probably the best simple criterion available.

Fig. 3 shows the coefficients of the moments obtained from the rudders of a variety of aeroplanes plotted against "rudder volume." The figure relates to 25° incidence of the main planes and a 20° rotation of the rudder from the neutral position. It is at once apparent that, with the exception of the Bristol Fighter and model A, points relating to the remaining models fall roughly into a band which is sufficiently narrow to suggest that if attention is paid to the features of design just enumerated, an approximate guess as to rudder power can be made from a knowledge of rudder volume. I have drawn my own idea of a good mean line through these points and it suggests that, with good design, a moment of coefficient at least 7 should be attainable from a rudder having a volume of 0.05, when it is moved through 20° from the neutral position. Wind tunnel tests show that the moment given by a rudder is nearly proportional to the angle through which it is moved, up to at least 30° from the neutral position; hence it would appear practicable to lay down the rough generalisation that a well designed and placed fin and rudder, capable of being moved through 30° from the neutral position, and having a "volume" not less than 0.05, should give a moment of coefficient 10. If the distance of the fin and rudder behind the centre of gravity is approximately equal to the semi-span of the wings, this

reduces to the very simple rule: Fin + rudder area 5 per cent. of the area of the wings.

It is again necessary to emphasise the point that the requirement of a moment coefficient 10, which can be met by a good fin and rudder of volume 0.05, applies to the conventional British biplane of thin wing section only. Other wing combinations may call for different rudder powers. There is reason to suppose, for instance, that wings which are tapered in chord and camber may call for smaller rudders, whilst wings having a large camber near the tip may require larger rudders.

I have said that given a rudder of sufficient power a reasonably alert pilot should be able to check the incipient spin in any stage of its development and restore his aeroplane to an even keel without using the ailerons. This statement

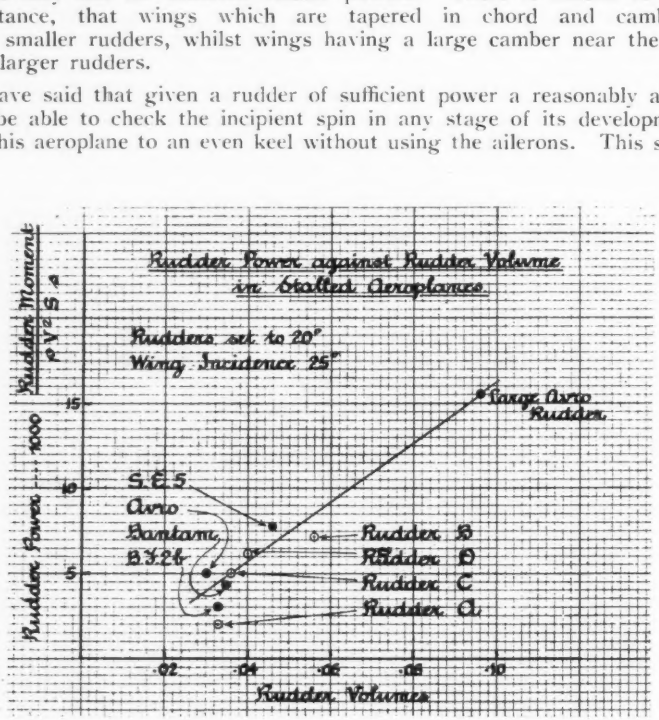

FIG. 3.

suggests the question—why then bother about special ailerons, since a large rudder is recommended for use in any case? The answer is that although a powerful rudder will enable the pilot to restore his aeroplane to an even keel and keep it there, no rudder, however powerful, will, in the absence of effective ailerons, give a satisfactory control over all the motions which he may desire to make. The action of the rudder on rolling, being indirect, is sometimes rather slow, so that a vigorous accidental disturbance, counteracted by the rudder alone, may result in an unpleasantly large roll, before the aeroplane can be brought back to an even keel. Again, the rudder can only be used to generate or counteract a roll through the medium of a turn or yaw in a specified direction, which may be an undesirable direction in which to turn. There is similar objection, though not so powerful, to the use of special ailerons unsupported by a powerful rudder. Special ailerons, it is true, can roll the aeroplane in any desired manner without help from the rudder, but they can only be used indirectly to turn or yaw it.

In general, it may be said that if an accidental or intentional stall during the approach to a landing ground is contemplated as a possibility, then it is important to provide the best possible control at that instant; if the movements required for that control can be made similar to those required in normal flight, so much the better. When a pilot stalls a conventional aeroplane with weak rudder, he feels as though he has lost both his ailerons and his rudder. Restore to him either of these organs, by providing either a special aileron or a powerful rudder, and he will be able to prevent the spin from occurring, but will feel as though he has lost the other organ. Restore to him both these organs and he will find that his control in stalled flight is not greatly different from that to which he is accustomed in normal flight. Finally it is worth noting that with the possibility of such a deadly occurrence as the incipient spin always present, there is much to be said for providing two entirely independent means whereby it can be prevented.

We have discussed the results which can be achieved by the use of a powerful rudder and special ailerons. Let us very briefly consider the question of cost in weight and performance. Taking the rudder first; if a rudder such as that

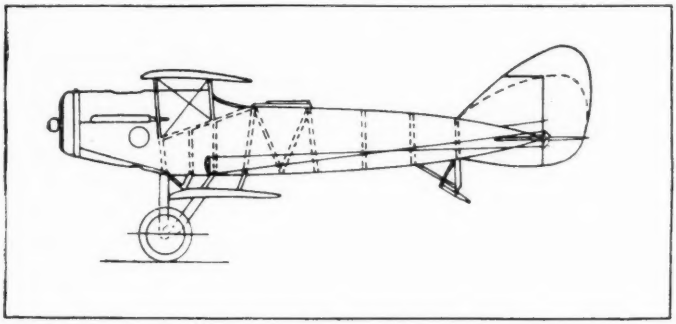

FIG. 4.

used experimentally in the Avro with a volume 0.096 were required, the cost would be very serious, but fortunately a much smaller rudder than this is sufficient; a volume of 0.05 being indicated, both by theory and practice. A rudder of approximately the latter strength was used on the standard S.E. 5, and is used at present on one of the most successful fighting aeroplanes. A crucial test of the matter was that of redesigning the rudder of the Bristol Fighter in accordance with these ideas. The new rudder* illustrated in Fig. 4 with the old rudder shown dotted for comparison has a volume of about 0.046 and has been universally reported, by all pilots of experience who have tried it, to effect a great improvement in the flying qualities of the aeroplane.

Turning to a consideration of special ailerons, Fig. 5 shows the particular combination of Handley Page Slot-and-Aileron which has proved so successful in flight. This, at first sight, does not look a very suitable arrangement to put upon a high speed aeroplane, but its adverse effect upon performance is not nearly so severe as might be supposed. The reduction of performance of the Avro for instance was within the probable error of modern full-scale experiment, that is to say, could not be positively observed. This device illustrated is, moreover, only a first attempt, in which neatness of design was given second place by comparison with the need that the experimental apparatus should work in an effective and striking manner. It is possible that for commercial work at

* Described in R.M. 972.

least, shorter ailerons and slots would be sufficient, and it should not be beyond the power of designers to modify the arrangement so as to make it neater, without destroying its effectiveness. But a word of warning is here necessary; such modification should not alter the relative shape of the various parts when the slot is open, unless wind tunnel experiments are contemplated, for the action of the combination is very sensitive to small changes in form.

These considerations lead naturally to a brief review of the researches yet required on this subject. First it is necessary to investigate more fully the minimum requirements, of rudder area and special ailerons, to give adequate control, both on the conventional British biplane and on aeroplanes of other

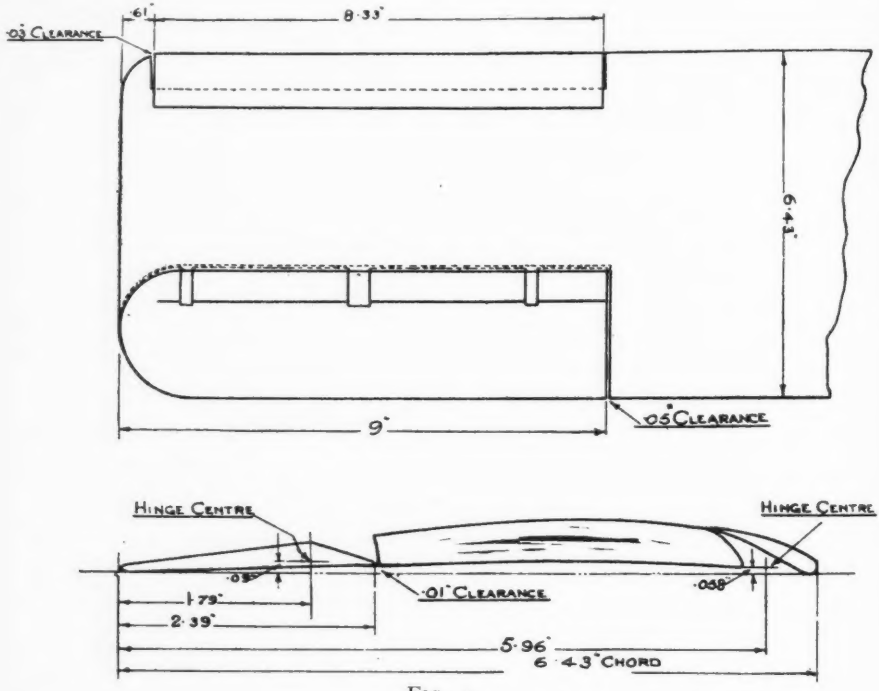

FIG. 5.

forms. We must know what happens with thick wing sections, with biplanes of various degrees of gap and stagger, with tapered biplanes and monoplanes, and with wings using devices, such as slots and flaps, to ensure unusually high lift coefficients. There is here scope for an immense amount of detailed research, both in the wind tunnel, to get detailed data, and on the full scale to determine what is adequate control.

Next, there is the question of "scale effect" to be studied. How far can we trust our wind channel data to represent the full scale? This is a thorny problem, the solution of which involves elaborate full-scale experiment with recording instruments of many kinds, all working simultaneously. As far as is known at present there is no indication of serious discrepancy between model and full scale. Deductions from model data have so far worked out well in practice, and the lift and drag curves for the Avro, a matter upon which much

depends, agree well up to 30° incidence on model and full scale. Experiments on scale effect are now in progress.

Finally the whole question of fore and aft control in stalled flight still needs study. Though not nearly so fruitful of accidents as the incipient spin, there is no doubt that aeroplanes designed in certain ways can be so manœuvred as to dive straight and uncontrollably into the ground, and the minimum requirements to prevent this from occurring need study. There is also to be studied the possibility of deliberately flying the stalled aeroplane right on to the ground, on occasions of serious emergency, without too seriously damaging the aeroplane and occupants. It is hoped to study this matter on some special research aeroplanes fitted with powerful and interchangeable controls and powerful shock absorbing under-carriages. Could this be done there would be a possibility of making a safe emergency landing on a commercial aeroplane in a field under 100 yards in length.

In conclusion, I wish to make some remarks relating to the time—five years or so—taken in reaching the results which I have been enabled to put before you to-night. When we reflect that many persons have been killed in that time by accidents which might have been rendered trivial or avoided altogether had the information been available earlier, it becomes a matter of some importance to consider how that time has been spent. The Aeronautical Research Committee did in fact realise from the beginning something of the magnitude of the research they were attacking and of the importance, from the point of view of saving life, likely to attach to definite results; they therefore adopted a policy of communicating to the Air Ministry, for publication or circulation to designers, all data, as it became available, and any definite conclusions which might be of value. This course has been followed consistently throughout; all available data was published in the Reports and Memoranda series as soon as its reliability was established, and the two main conclusions as to importance of rudder power and the effect of the slot and aileron were communicated to the Air Ministry at the earliest possible moment after they were considered to have been established. I even find that a reference to the possibility of the value of the Handley Page slot was included in my lecture to you in 1923, in the hope that someone able to work more rapidly than a Government department might be able to make practical use of the idea.

To criticisms that the whole of this work might have been completed in a year or so I am personally quite indifferent; they arise from ignorance of the difficulty of the problem and of the nature and difficulties of pioneer research. I do not believe that under any circumstances it would have been practicable to have worked through the mass of detailed experiments required to obtain the data on which to think, through the amount of thinking required, and through the full-scale experiments necessary to check the results of that thought, in less than some three or four years. But I do feel sure that the present stage could have been reached some year or more earlier, given a perfect organisation and entirely adequate workshop and experimental facilities. The determining factor in regard to speed has been, and always is, the full-scale work, and the determining factor in the full-scale work is the rate at which experimental alterations can be made to the aircraft. I have no hesitation in saying that in this respect the research has gone slower than it need have done. For this I do not blame the Royal Aircraft Establishment, which has had to make the alterations; still less do I blame the experimental staffs concerned, who have worked at the research in a manner which must command the unqualified admiration of anyone knowing the whole facts. The blame I think must be laid mainly upon the economy campaign which raged some years ago, and I only mention the matter at all to draw attention to the fact that what may appear to be economy at the time, may in fact prove to be a colossal form of extravagance. How much money would we give now if we could go back and undo the Croydon accident and the service accidents of the

last year, or at least make them non-fatal? In my own view the Croydon accident and a large proportion of the fatal service accidents of the past year, would not have ended fatally had the pilots been equipped with aeroplanes which they could control adequately after stalling.

May I attempt to summarise the situation as I see it. The majority of fatal flying accidents of the past have been rendered fatal by the incipient spin following an accidental stall near the ground. The reasons for this spin and the pilot's failure to check it are now thoroughly understood, and two independent ways of enabling him to check it are known. These are (1) a rudder somewhat more powerful than has been usual in the past, and (2) a special form of wing tip control. If both these modifications are adopted, stalled aeroplanes become, in many respects, as controllable as aeroplanes in normal flight.

Discussion

General Brancker said: With regard to this most interesting paper, I have one point which I should like to make, and that is that although Professor Melvill Jones speaks of the "common" Handley Page flap, I never seem to see this device on new machines. One of my favourite sayings is that theory follows rather laboriously after practice, but here it is scientific knowledge which is ahead of practice. I have been to see the drawings of an experimental machine recently, which I had hoped would be fitted with these up-to-date improvements; but I found the wing section was of exactly the same form as that of aeroplanes we have known for years. I asked the designer why he did not give us something new to improve his performance such as slots, flaps, new wing sections, and so on; he first told me that the Air Ministry would not like them because they did not care for new ideas. I denied this hotly, and he then told me that these devices were really not worth while because of the extra weight and other complications involved. Really it seemed that he had not seriously considered them. I have hopes that after our conversation he will try to make use of them. Some of our designers do seem to be a little slow in taking up new ideas. I feel that they are afraid of making failures and so losing reputation and goodwill with the Air Ministry. I hope someone will challenge this opinion.

Major F. M. Green said: Mr. Chairman, Ladies and Gentlemen,—I should like to thank the lecturer very much for his paper, and particularly for the "semi-philosophical remarks" which are in the printed paper but which he did not read. He has expressed a point of view admirably, and in future no engineer will want to quarrel with a scientist unless he wishes to be considered one of the less intelligent class. I can perhaps explain some of the reasons which have the effect of delaying the putting of new ideas into practice. As an example, we should all like to use big rudders, but we have to think of the result on the performance of the aeroplane. Unfortunately the airworthiness regulations say that a certain load per sq. ft. must be carried on the rudder irrespective of its size, consequently a bigger rudder means an increase of weight nearly proportional to the increase of area. It might be more reasonable, particularly in large aeroplanes, to fix the total load on the rudder irrespective of its size, as is the rule for tail planes. In other countries the airworthiness regulations demand a certain load per sq. ft. on the tail plane, with the result that aeroplanes made in these countries as a rule have much smaller tail planes than we consider necessary. An alteration in the regulations applying to rudders would encourage designers to use bigger rudders, and I think this might be done without any fear of the rudders not being strong enough.

There is another reason why designers do not experiment immediately on the lines suggested by new inventions. The Government has the right of user

of all inventions and in consequence a designing firm gets little or no advantage for itself by the development of a new invention and is therefore unwilling to incur the expense and risks involved without Government assistance. The undoubted merits of Mr. Handley Page's slotted wing have not yet been realised, probably because enough time has not been spent on evolution of a design which reduces the device to its simplest and lightest form.

Mr. HANDLEY PAGE said: I am in a somewhat privileged position this evening, because I feel that I am not one of those people who in this matter can be accused of lagging behind in the progress of aviation. On page 353 the lecturer mentions that he gave some idea of the possible value of the slotted wing in 1923. I may perhaps claim a little priority over the lecturer in that I had some idea as to its value in 1919 and 1920.

There are two aspects which we have to study; one is the question of preventing the stall actually taking place. The completely slotted machine will do this. I suggest to the lecturer that one could have a completely slotted machine with an emergency handle to let go when in a stall. The resultant forces on the forward aerofoil are such that the forward aerofoil should open automatically when you approach a very large angle of incidence.

Secondly, there is the question of lateral control. Lateral control can be obtained in one of two ways. In a non-slotted machine the slots interconnected with the ailerons are opened on that side of the wing which is depressed and on which extra lift is required. The slots in the rising wing are kept closed. With the fully-slotted machine the reverse control, if one may term it so, is the better. In this type the slot is closed on the higher wing and thereby the lift is decreased and the resistance increased. From experiments which we have made it would appear that the lateral control is better in the latter case.

The lecturer has not been able to refer to the latest results that have been obtained, and it is not possible for me to give full details in regard to a machine on which they have been obtained. Briefly, however, it may be stated that it is possible to use the type of slot which closes completely up and leaves no gap under the plane in the closed position. There is therefore no loss due to this new type of control causing an increased resistance. This type of slot has been used in conjunction with slotted ailerons. With a combination of slotted aileron and forward aerofoil, complete lateral control at the stall has been obtained on a wing whose maximum lift coefficient is approximately .6 with a loading on the planes of between 11 and 12 lbs./sq. ft. The maximum lift coefficient with both slots open is between .9 and 1.0. I mention the high loading of the planes as therein lies the difference between this machine to which I refer and the Avro which was demonstrated at Croydon. A criticism directed against the slotted control of the Avro is that the Avro is extraordinarily lightly loaded, say, between 4 and 5 lbs. per sq. ft., and therefore the control that one can obtain with such a machine is no criterion of what would happen on a machine of 12-13 lbs./sq. ft. loading.

I hope that we may have the Chairman's assistance in obtaining permission to publish some of the details of the machine, which will be of use to people who are contemplating employing the slotted control.

It will be possible without the interposition of a cam device to link up the aileron and front aerofoil, and obtain the necessary movement without this mechanism. A cam introduces a certain amount of friction which tends to increase with high loads, and the elimination of it seems to be a possible improvement.

I should like to thank the lecturer very much for the research work which he has done, and further, to offer him an opportunity of flying on certain machines

at Cricklewood, and helping in the research work. We should be only too pleased to welcome him at the earliest possible moment.

Major BUCHANAN said: I think aviation as a whole is very much indebted to the lecturer for what he has done on the control of aeroplanes in the stalled state. Like several other speakers in this discussion, I was very much interested in his "semi-philosophical" remarks.

The two points of rudder control and aileron control may be discussed separately, although at the same time they are interconnected. A large proportion of the modern aeroplanes have the rudder volume specified by Professor Melvill Jones, but very few modern aeroplanes have lateral control anything like the standard that has been set down in his paper. Is this amount of lateral control absolutely necessary? The mechanical problems connected with the combined operation of slot and flap are considerable. There is more aerodynamical resistance and more weight. Some tests have been made, and as far as I can see the loss might possibly amount to 8 m.p.h. The additions to the structure do not make for cheapness of running costs, and engineers and designers, who have to weigh cost against possible improvement before entering on an enterprise of this kind, are naturally in some doubt. So far as the Air Ministry is concerned, we have been watching these experiments with great interest, and have from time to time sent out all the details of the research work done to the aircraft industry as a whole. At the present time the information available has been put before all firms, and they have been asked what they consider the best way in which these types of control can be used.

With regard to rudder control, it is proposed that no aeroplane be accepted for service or commerce without at least the volume of control specified by Professor Jones in his paper.

Captain G. T. R. HILL said: Mr. Chairman, Ladies and Gentlemen,—I should like to congratulate Professor Melvill Jones and his colleagues on the immense energy with which they have pushed forward this research work, and on the satisfactory results they have achieved.

I also was very much taken with his "semi-philosophical remarks," as I think it is important to show that the x's and y's do mean something. It is a point which will appeal to the general public, and after all it is the general public's money which enables these researches to be carried out.

The lecturer asked for any information as to crashes on the S.E.5 as a result of an incipient spin. I have always understood that McCudden was killed in this way. I believe he spun from about 200 feet into the ground.

One point I should like to raise, namely, the longitudinal control when stalled. I will read you a few sentences from R. & M. 963, which was written by my brother and Mr. Stevens. "The aeroplane was . . . pitching and rolling violently. During the positive pitch, the pilot pushed the control stick full forward and moved it rapidly back in anticipation of the negative pitch, but for some time without any visible effect. Then, during one of the times, when the nose was rising up, he managed to catch it and prevent it from rising. The oscillation being damped, he was able to force the aeroplane into a steady dive. All this shows that the elevator control reserve was very small; and, as always happens when the control reserve is reduced so much, it was mainly a matter of the pilot's skill to make any use of it at all." That does not make one feel very happy, and I should like an assurance that this difficulty has been either overcome or fully appreciated.

As a result of some of my own experiments I should like to emphasise the importance of informing the pilot as to when he is stalled. At present with the ordinary aeroplane you know when you are getting near the stalled point because your control disappears. With improved control you will not be upset,

but you are definitely going down the whole time at considerable speed when you are stalled, and it is not a simple matter to reduce that vertical component of speed because you must push your nose down to pick up more speed before you can get back to your unstalled mode of flight, with its reduced vertical velocity, and with the power to flatten out the flight path to land.

Turning now to the question of safety in general, I admire the way in which the lecturer takes his stand regarding the degree of safety we have reached nowadays. I quite agree with him that flying at present is not really safe enough to enable one's wife and children to use it as an everyday means of transport. I know that there are plenty of people who will contradict that, but when the man in the street is told that flying is absolutely safe, while the next day he reads that there have been fifty-seven accidents in the Air Force in the last year, he does not know where he stands. He feels in the back of his mind, and rightly, that there is danger, and we must face this danger and work to reduce it. I do feel that the difficulties are crumbling in our hands as we work, and the time is in sight when the future of aviation will be very much brighter by reason of a great increase of safety in flight.

Mr. BRAMSON said: Unfortunately I arrived late enough to miss the beginning of the lecture, and did not hear an actual definition of the stall. It has been described as the point at which the machine gets out of control, but is it not right to define it as the point at which any manœuvre which you make with your joystick will make the machine sink more rapidly than it did before? That leaves one with the question: Is it ever desirable to fly in the stalled state? Except in stunting, I think it is not, and that brings us to the further question: Is it ever desirable for a pilot to get to the stalled state without knowing it? That is a totally different problem. Assuming that you are flying low—if you are not flying low, stalling does not matter so much—and in difficulties, worrying, and wanting to make a forced landing. Now suppose that you could stall without the stall involving loss of control and you could still fly on an even keel; if you are over difficult country, stalling would mean sinking rapidly just when it is most necessary that you should have every foot of altitude available for manœuvring and for getting down to the place you want to land on, and that is why, to my mind, the problem of rendering an aeroplane controllable after stalling is an entirely different one from that of making it certain that a pilot should know when he is on the point of stalling. That problem has been tackled from a different point of view, namely, from that of warning him. For any given machine there are many different stalling speeds, so that the air speed indicator does not always help, and may be a danger. The problem has been tackled from the point of view of giving the pilot a warning whether he wants it or not, and in the only infallible way through his sense of touch. The object of the device is to convey to the pilot through his sense of touch an unfailing warning that he is about to stall. On different days the stalling speed of the same machine is different, for various reasons, such as a light or heavy load, static or dynamic. A warning therefore must be given in terms of incidence, the stalling incidence of any given machine being constant, irrespective of loading. An unstable vane hinged behind it and set at an angle to chord of one or two degrees less than the stalling angle is exceedingly accurate in action, and will give an unfailing warning of an impending stall. In the actual device which has been made and tested by Air Ministry pilots and others, the type of warning is simply a sudden application of force on the joystick in the direction in which the stick ought to be moved to prevent the stall occurring. I submit that even though the problem of making aeroplanes controllable when stalling be solved, it should be impossible for an aeroplane to reach that stage without the pilot knowing about it.

Mr. H. E. WIMPERIS said: Professor Melvill Jones is a master of the subject

on which he has lectured to us this evening, and it is a great privilege to be able to listen to him. Not only has he knowledge of the matter, but he has spent a great amount of time in obtaining first-hand knowledge of it. It is due that I should express the thanks of the research staff engaged upon this work for the very considerable assistance he has given as a volunteer worker on this problem. The research is by no means at an end, but it does from time to time reach an interim stage when a résumé of the results achieved may be given to all who are likely to be interested, the aircraft constructors particularly. A number of such interim reports have been issued. The work, however, is still going on, not only along the lines you have heard of to-night, but in addition a study is being made of the Fokker machine, which demonstrated so notably its qualities of stability and control. Work is also being done on the Hill tailless machines. Captain Hill did not mention this, so I will do so for him. His machine is quite unlike any ordinary machine. The rudder control is placed on the outer wing tips and there is no position in which the body can blanket this control. In effect, much more control is obtained at low speeds by the Hill method. That is all I can say about it because the official trials are still going on.

There are quite a number of other devices for attaining control at low speeds which I would like to mention, but they have not yet reached a stage at which it is possible to discuss them. These researches are limited only by reasons of finance and the fact that there are other things that have got to be done also. The work is, however, being given a high priority.

Major C. K. COCHRAN-PATRICK said: First of all I should like to thank the lecturer very much for his extremely interesting paper.

I think I can answer to some extent one of the questions he asked. With regard to S.E.5's, I remember, during the war, once in 1917 on an aerodrome in France collecting in the space of a fortnight no less than three S.E.5's that had spun into the ground from a low height. They had been piloted by new pilots just out from England, and it is quite possible that the accidents happened owing to the fact that the pilots were inexperienced. Major McCudden flew into a tree; he did not spin into the ground. This point leads us on a little further. The lecturer mentions at the beginning of the lecture the danger of the incipient spin catching the pilot off his guard, and I have been trying to recall cases of a pilot spinning into the ground. As far as I can remember myself, no experienced pilot has spun into the ground and killed himself on a machine which did not flick into a spin. Machines can get into a spin in very different ways. De Havillands and Avros and big machines get into a spin fairly slowly, and you have time to realise the danger and stop it, though you cannot stop it without losing height and diving. Others definitely flick into a spin. The Sopwith Camel and Spad go into a spin so quickly on occasions that even an experienced pilot does not realise it until he has done half a turn. The moment you realise a machine is in a spin, you can stop spinning in half a turn.

Curiously enough, machines which flick into a spin stop easily. Having stopped a machine spinning, you then have to bring it out of the dive which follows and in that process you usually hit the ground. I suggest that the lecturer should concentrate on elevator control in a stall. I do not know anything about the aerodynamical problems concerned, but it seems to me that the reason why you cannot bring a machine quickly out of the dive which follows a spin is that the machine is not going fast enough to give the elevator sufficient control. If you could make your elevator sufficiently powerful to at least alter the angle of the machine, you would hit the ground with your undercarriage instead of end on.

Probably what is necessary is something which will definitely tell the pilot that the machine is about to stall, and then give him control when it is stalled. The combination of the two would, I believe, eliminate fatal accidents.

Mr. H. B. IRVING said: On this question of spinning it is important to realise that it is not yet proved that the slot and aileron control will by itself definitely bring *all* machines out of spins. I think Professor Melvill Jones himself is not at all confident that it will. He mentioned that it would bring the Avro out of a spin. Now it is generally known that when an aeroplane is spinning there is a variation of incidence along the span. That variation depends on the speed of the spinning. Professor Jones mentioned that the variation is, roughly, from 0 to 80 degrees. This is probably an overstatement: the Avro does not spin very fast, and probably 0 to 40 degrees rather than 0 to 80 would more nearly represent the case. We know from model experiments on the slot and aileron control that at about 40 degrees incidence the control begins to fall off. We do not know what happens when the incidence is as large as 80 degrees, but if the above facts are borne in mind they will make us realise that in a fast spin, where the maximum angle of incidence may reach 80 to 90 degrees, the slot and aileron control may not be sufficient to stop the spin.

I was very interested in Major Cochran-Patrick's remark that machines which go easily—or "whip"—into a spin come out easily, and *vice versa* with those that are reluctant to spin. The B.A.T. "Bantam" is a case in point. This aeroplane went into a spin slowly and gradually settled down to a fast "tight" spin from which recovery was difficult.

As regards the various ways in which ailerons and slots may be used in combination, mentioned by Mr. Handley Page, I should like to state that the possibilities in this direction were kept in view at the time the preliminary experiments on the slot and aileron control were made at the National Physical Laboratory, and the experiments were accordingly arranged so as to cover all the possible cases. Some model data in this connection have therefore been available to the public for a considerable time.*

I should like to add my thanks to Professor Melvill Jones for his paper.

Major HEMMING said: I should like to record my great appreciation of Professor Melvill Jones's paper and of what he has done in carrying out these valuable researches.

Professor Melvill Jones is one of our very few practical scientists. All those who were at Orfordness during the war will remember how he used to fly before he was actually given his wings. I well remember how he used to go up as a passenger and encourage the pilot to take him into the middle of black clouds, in order to solve the problem of flying in clouds. Knowing the very practical way in which he does his work, one attaches all the more importance to his views on the researches which he has carried out.

The question of control while in the stalling position is most important from the commercial point of view. In Europe we cannot say that air transport is the safest mode of travel. In the Colonies, however, the air method is safer than many of the other normal methods. If one can crash one's machine safely—in other words, pancake into a small area without damaging the passengers or seriously damaging the machine—one can at once see many parts of the world where air transport becomes an immediate possibility. At the present time air transport depends on subsidies. I understand that only one air line in the world is worked without a subsidy. One way of doing without subsidies is to spend money on research in order to produce a commercial aeroplane, and when once that machine is produced, people will automatically come forward to operate and finance air lines equipped with such machines. I think if we can get the public to agitate for funds to be devoted to research work we may get more definite help for civil aviation than by paying subsidies. Once we can assure the man in the street that we can take him from A to B as safely as by any other mode of transport, air transport will become self-supporting.

* See Aeronautical Research Committee, R. & M. 866, February, 1923.

Colonel FULLERTON, R.E. (*communicated*): The lecturer has given a very interesting account of the efforts made to solve this question, and his resulting equation should be very useful to the designers of present-day types of aeroplanes. I should, however, like to point out that what is really wanted is some sort of arrangement which will *prevent an aeroplane stalling at all,* unless it is made to do so by the pilot.

It seems to me that we can learn something in this direction from the birds, which apparently never stall, or if they do can recover themselves remarkably quickly. They have no rudders or fins, so there must be "something" in their construction which prevents, or at all events modifies, stalling.

I believe this "something" depends upon:

1. The flexibility of the outer parts of the wing area, which makes these parts act as automatic "ailerons."
2. The peculiarity of construction which enables a bird to move its wings slightly backwards, so as to bring their centre of pressure in rear of their centre of gravity, a motion which at once causes diving, and brings them out of a stall.

As regards the wings, nearly two-thirds of their areas are flexible, and they can by suitable muscular action alter their cross-section from a stalling to a diving position at will (see JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY, "Flight of Birds," Fig. 2, October, 1925, and same Journal, p. 595, para. (1), line 3).

Next, consider the wing motion described above. It is clear that the backward motion of the wing alters the balance and causes the bird to dive, while if it wishes to stall, it has only to advance the wings to enable it to do so. It seems to me that both these ideas are worth considering, as the prevention of stalling is one of the most urgent aeronautical questions of the day.

REPLY TO DISCUSSION.

Professor B. MELVILL JONES: This has been an intensely interesting discussion and my difficulty is to reply adequately to all the important points which have been raised. I think that before replying in detail I should lay emphasis upon the fact that my delivery of the lecture to-night does not mean that the researches which I have described are all my own work; they have been carried out by a great number of experimenters directed by a small panel, to which it has been my privilege to belong. A lecture can, however, be delivered only by one man at a time, and it has fallen to me to do it.

General Brancker's difficulty, that designers have not yet used the slot and flap to increase lift, is due I think partly to lack of precise information upon the full scale and partly to the fact that it is much easier to put up loading to the required figure by raising the stalling speed than by fitting additional apparatus to the wings. The first difficulty has now been partially remedied. The remedy for the second is to place a regulation limit on the stalling speed of passenger-carrying aircraft; if this limit were not too high, the desired result would automatically follow.

I am glad that Mr. Green and others approved of my "semi-philosophical remarks," but his forecast that "no engineer will want to quarrel with a scientist" is, I think, optimistic. Such a state of affairs, if it came about, would be very bad for the scientists; an occasional attack, just or unjust, upon our habits of thought is all to the good. I appreciate Mr. Green's difficulty in the incorporation of large rudders into designs, and I trust that steps will be taken to alter the regulations so as to avoid this difficulty.

Mr. Handley Page, I think, mistook the meaning of a remark in my paper; obviously my reference to the slot in 1923 was not the first reference to slots. When, in 1923, I drew attention to the fact that our researches suggested that

the slot, the function of which was well known at the time, might be of great value in connection with control after stalling, it was not with any idea of establishing priority, but in the hopes that somebody might be encouraged to develop the slot for this purpose more rapidly than we expected to be able to do ourselves. I agree with Mr. Handley Page that it is important to use a slot all along the middle part of the wings in order to delay the stall, together with a slot and aileron at the tip for control, and that then control can be obtained either by closing the slot on the tip to be depressed, or by opening it on the tip to be raised. We realised this a long time ago, but desired to concentrate first on control before proceeding to complicate the problem by slotting the middle part of the wing. A Bristol Fighter is now undergoing alteration which will allow a combination of slot and flap over the middle of the wing to be used with a slot and aileron control at the tips, operated in either of the above ways. The preliminary wind tunnel experiments are almost completed. I thank Mr. Handley Page for the offer of flights on his experimental machines at Cricklewood, and shall certainly accept his offer as soon as I have time to do so.

In answer to Mr. Buchanan, I do not know of the tests which have led to an estimate of a reduction of 8 m.p.h. on top speed due to using the slot and ailerons; I think it is an overestimate, even with the existing arrangement which, on the Avro, did not make any measurable difference to top speed. Possibly he refers to slots all along the wings on racing aeroplanes. But in any case, it is only a matter of time and ingenuity to devise a slot gear which will fold down until the wing has a fair form. In regard to fixing the minimum size of rudder by regulation, I think this is a wise step to take, but it should be borne in mind that the " rudder volume " 0.05, given in my paper, is merely a round figure suitable to conventional British biplanes, and might be either too small or larger than necessary in conjunction with other forms of wing.

I can assure Captain Hill that the problem of longitudinal control has not been overlooked, the attack upon it merely postponed until the lateral control had been cleared up, the latter being, from the records of accidents, the most urgently required information. The problem is now under consideration. I agree entirely with his opinion that flying is not yet safe enough for wide commercial expansion, but that it is on the verge of becoming so. Many factors are working towards this end; as I have tried to explain to-day, methods of overcoming the chief dangers inherent in stalling are becoming available, and such developments as the Savage-Bramson stall indicator, the three-engined aeroplane, the leader cable, and by no means least, Captain Hill's own tailless aeroplane, upon which he will lecture next session, are all tending in the same direction. I would have referred to this last in more detail in my lecture, but I did not want to steal his thunder; he will tell you about it next session much better than I could.

I agree with Mr. Bramson's definition of the stall and with his remarks about the necessity of having a stalling indicator, and I consider, from the reports I have read, that his device for this purpose should be of the greatest value. I have no doubt that ultimately something of this kind will be a standard fitting upon all aeroplanes; we want both to be warned when an aeroplane is about to stall and to retain control of it after it has done so. I am not so certain as he is that deliberate stalling when landing will never occur; it may become a most convenient way of losing height quickly, once the sting of loss of control has been drawn.

I thank Major Wimperis for his appreciation of my efforts to assist in the work of elucidating these problems of control, and for his clear statement of the situation. I can assure him that my remarks about lost time in research were not directed against him, but rather towards those who may again contemplate an undue curtailment of research at any time in the future.

Major Cochran-Patrick's remarks were exceedingly interesting to me. His experience is enormous, and what he says on war flying has the greatest weight. I should like to go further into the question of these S.E.5's which span rapidly into the ground. The whole question of the way in which machines go into a spin is being examined, and very interesting results are being obtained from wind channel experiments. Different arrangements of wing as biplanes, and different wing sections, can produce different characters of spin, and it is quite likely that much research is required to understand them. We are only at the beginning of this problem. I agree that on most aeroplanes—not on all—a spin can be stopped in half a turn, but if the rudder is weak this can only be done by the elevator, with a consequent loss of height. A powerful rudder enables the spin to be checked without so much loss of height, since the stick need not be put forward, but the action is still not so rapid as is desirable and some modified form of aileron, such as that I have described, is necessary to get a quick and positive control. The response of the aeroplane to this control is very rapid. With regard to elevator control, I have already said, in reply to Captain Hill, that the focus of the research is now shifted to this problem.

In reply to Mr. Irving, I did not mean to imply that the slot and aileron will necessarily stop a fully-developed spin on all aeroplanes, but that it will do so on the Avro. As he says, there are some aeroplanes—not the Avro—in which the spin may be so fast that the incidence of the inner wing may reach 80 degrees or over. The slot and aileron will probably not stop such spins when fully developed, but since they take some time to develop, this does not affect the usefulness of the control for ordinary stalled flight.

I thank Major Hemming for his remarks; at this distance of time I do not mind admitting that the cloud flying to which he refers gave me a peculiarly frigid feeling about the feet. I thoroughly agree with him that commercial flying will become a successful proposition when, and only when, the right type of aeroplane is forthcoming, and that it has not come yet. I also agree that more and still more research is required to make such a machine possible, but I think that, given sufficient research and the corresponding work of development in the private firms, there is a distinct prospect of such a machine being forthcoming in the near future.

In answer to Colonel Fullerton, I imagine that the use of a Handley Page slot all along the wing, coupled with a Savage-Bramson stall indicator, will practically prevent accidental stalling, but on top of this it is as well to retain control after the stall. I do not agree that birds never stall; it was watching what appeared to be the deliberate stalled landing of certain seagulls some years ago that encouraged me to think that the same thing might possibly be done by aeroplanes. However that may be, I agree that they rarely stall by accident, but considering their much greater experience and more direct nervous connection with the different elements of their wings, this is scarcely to be wondered at. I think it is highly probable that the fore and aft control of birds is mainly affected by forward and backward movements of the wings. This method I imagine should be most effective, but in an aeroplane would involve a very serious structural complication and a great amount of inertia in the control. It is worth noting that Captain Hill's aeroplane achieves apparently very good control, both before and after stalling, without the use of a tail or rudder in the ordinary sense; the organs which perform the function of these controls being situated at the wing tip.

Sir SEFTON BRANCKER, in proposing a vote of thanks, said: We have listened to a most valuable paper, and a very interesting discussion. We must not only thank Professor Melvill Jones most warmly for his paper, but also for his invaluable services to British aviation during the last fourteen years.

PROCEEDINGS

FOURTH MEETING, SECOND HALF, SIXTY-FIRST SESSION

A meeting of the Royal Aeronautical Society was held in the Society's Library, at 7, Albemarle Street, London, on Thursday, February 16th, 1926, when Flight Lieutenant H. Cooch read a paper on "Landing Aeroplanes in Fog."

Wing Commander T. R. CAVE-BROWNE-CAVE, C.B.E., F.R.Ae.S., who presided in the absence of Sir Sefton Brancker, said: Flight Lieutenant Cooch is going to describe to you the work he has been doing at the Royal Aircraft Establishment to evolve an instrument which will enable a pilot who arrives at an aerodrome in fog to determine his height and position, so that he can land.

Flight Lieutenant Cooch, during the war, served under me on airship construction in charge of the electrical apparatus used. He devised the apparatus and instruments, installed them and, what was often more difficult, taught the pilots to use and trust them. When airships were closed down in 1921, he joined the Royal Aircraft Establishment, where he was set to solve the difficult problem about which he will speak to-night.

LANDING AIRCRAFT IN FOG

BY FLIGHT LIEUTENANT H. COOCH.

1 *Introduction*

There is no need for me to mention to members of this Society the difficulties of landing aircraft in fog or to point out the advantages which would accrue from a solution of these difficulties.

The object of this paper is to give briefly an account of the work which has been done on the subject by the Electrical Research Section at the Air Ministry and later in the Electrical Department at the Royal Aircraft Establishment, Farnborough.

Investigations were commenced by Major McAlpine, Head of the Electrical Research Station, Air Ministry, as early as 1921, and for the past four or five years work has progressed steadily and we have now reached a stage when full scale trials can be commenced with a very good chance of success.

The scheme is an electrical one, and its description must contain a large proportion of electrical and magnetic details. I do not propose to go deeply into the technical side of these details, but rather to show as simply as possible how and why the scheme operates with a view to interesting, and drawing criticisms from, those connected with aviation.

2 *The Problem*

In the problem presented to us it was assumed that the aeroplane had been guided to the vicinity of the aerodrome and required a means for landing safely from a height of about 1,000 feet when the ground was obscured by fog or mist.

At first glance it would appear that the problem is to provide a means for indicating very accurately the height from 1,000 feet downwards when over the aerodrome. There is, however, another very important point in the problem which is perhaps not so generally appreciated. When a pilot is about to land on an aerodrome he must know his position with respect to the centre of the aerodrome as well as his height above the ground.

With the limited space available on most aerodromes it is quite usual for the pilot to start his final glide to land when over ground well outside the

aerodrome. If the ground is invisible, the pilot must have instantaneous and accurate indications of his position as well as height.

When this problem was first tackled it was decided that an endeavour should be made to provide an indicating device which would give direct readings of height and position without calculations of any sort. It should be embodied in an instrument which could be conveniently carried on the aircraft and which would require no technical skill in operation.

We were not limited, within reason, to any definite size of ground plant, but the size and weight of the apparatus carried on the aircraft had naturally to be kept as low as possible.

The Magnetic Field Produced by an Electric Current

It is well known that if an electric current is passed along a cable a magnetic field is produced in the form of concentric circles around the cable. If this cable is laid just below the surface of the ground, the magetic field above the ground will be in the form of semi-circular arches extending along the whole length of the cable. The strength or density of this magnetic field varies inversely as the distance from the cable. The direction of the magnetic field passes through the horizontal at all heights in positions which are vertically above the cable.

If this cable is laid as a track on the aerodrome, then at all heights there will be an invisible track of horizontal magnetic lines, and the shape of this track will be approximately the same as the shape of the track of the ground cable.

Here, then, we have a means of producing invisible tracks of practically any shape; they are present at all heights and their density is inversely proportional to the height. If we can find a means for giving indications to the pilot so that he is able to follow these magnetic tracks and at the same time give the height above the ground by measuring the magnetic density, we have gone a long way towards solving our problem. It is these simple facts that have encouraged us in our efforts to find a means whereby an aeroplane can follow these tracks and to obtain an instrument to measure their density.

It should be noted that the density of the field is inversely proportional to the height which gives us a stronger field near the ground or in the position where most definite indications are needed. I should like to make a comparison between this medium for indicating height and the ordinary atmospheric pressure medium. With the magnetic medium the densities at 10 feet and at 1,000 feet are in the ratio 100 to 1; with the barometric method of measuring height we have a medium at 10 feet and 1,000 feet which varies only in the ratio of about 100 to 97.

The idea of using an electric cable carrying alternating currents for the purpose of assisting navigation is by no means a new one and much valuable work has already been done on the subject. This method of navigation is generally known as the "Leader Cable System," and descriptions have from time to time appeared in scientific and technical papers.*

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* "Induction Through Air and Water at Great Distances Without the Use of Parallel Wires;" "Telegraphic Communication by Induction by Means of Coils." Stevenson. Proceedings of Royal Society of Edinburgh. Vol. 20, 1893, 1894, pp. 25 and 196.

Technical Notes on the "Leader Cable System." Admiralty Signal Department. G.15630/20, March, 1921.

"Modern Marine Problems." (Kelvin Lecture.) Drysdale. Journal of the Institution of Electrical Engineers. Vol. 58, July, 1920.

"The Distribution of the Magnetic Field and Return Current Round a Submarine Cable." Drysdale. Phil. Transactions of the Royal Society. Series A, Vol. 224, pp. 95-140, 1924.

Ditto. Butterworth. Phil. Transactions of the Royal Society. Series A, Vol. 224, pp. 141-184, 1924.

"The 'Loth Leader Cable System' for Electrical Steering of Aeroplanes." Gray. The Institution of Aeronautical Engineers, Proceedings No. 9, 1923.

In our problem of landing aeroplanes in fog we have made use of a " Leader Cable " in the form of a closed loop around which an aeroplane can travel, and we have endeavoured to obtain an instrument which will not only enable the aeroplane to follow the track of the cable, but also to simultaneously record the height above the ground by measuring the density of the magnetic field.

Measuring the Density of the Magnetic Field

This, I may say at once, is the crux of the whole problem.

I do not propose to give you details of all the various methods which have been suggested and investigated, but I will give you a few points which will enable you to appreciate the problem and note our progress towards a solution.

At the commencement of our investigations it was agreed that the current in the ground cable ought not to exceed 250 amperes. The ground plant to produce this current would be, roughly, 15 h.p. per mile of track. The weight of search coils carried by the aircraft should not exceed, say, 10 lbs.

The most convenient method of measuring the strength of an alternating magnetic field is to place a coil of wire, or search coil, in the magnetic field and then to measure the electromotive force induced in the search coil. The amount of power picked up by the search coil depends on the density and frequency of the alternating field and on the size and weight of search coil.

The density of the magnetic field at 1,000 feet above a cable carrying 250 amperes is about 1/100th of the density of the earth's magnetic field.

A navigation compass, therefore, is operated by a magnetic field which is a hundred times stronger than the field obtained at 1,000 feet above a cable carrying 250 amperes.

With this current in the ground cable alternating at 33 periods per second, the amount of power we could expect to pick up in the search coils on an aeroplane at 1,000 feet would be somewhere about $\frac{1}{4}$ microwatt. This is the power that would be available to cause a suitable deflection on our instrument.

In searching for an existing instrument which would measure a power of this order, it was found that the usual commercial instrument took something like 100,000 microwatts or 400,000 times more power than we had available. The best instrument that could be obtained and which might possibly be carried on aircraft took about 2,000 times the available power.

It was quite clear that the scheme could not be made a success if we had to depend on existing instruments and our energies were concentrated on finding some method of measuring these extremely low powers. Instruments suitable for laboratory use could be obtained, but these would be useless on aircraft.

Apparatus is available for detecting powers of this order (the telephone receiver for example) and investigations were carried out with a view to making these detectors act as measures.

A detector, which had been designed for operation in connection with the leader cable scheme at sea, was very kindly lent by the Admiralty Research Laboratory. This detector, when set to withstand a reasonable amount of vibration, was capable of closing an electric contact with a power consumption of 0.3 microwatts. Two of these detectors were embodied in our first model instrument and these controlled moving switch arms in such a way that the strength of magnetic field was indicated by the number of electric lamps illuminated. I need not go into details of the construction of this instrument, but the model is here and can be examined by anyone interested.

This instrument, which took a considerable time to design and construct, was ready for trial on a small scale model scheme by about the summer of 1923. It failed in one very essential point, namely, that it could not answer quickly enough to the changes in density of the magnetic field due to rapid changes of

position of the aeroplane. It, however, served one very good purpose in showing that the scheme generally would be a success if we could obtain a means for measuring these weak magnetic fields without time lag.

At this stage we really had to begin over again, and as a result of further efforts an entirely new method of measuring alternating currents was devised. This method was the result of the following very simple line of argument. It is well known that a direct current instrument absorbs very much less power than an alternating current instrument; also that an instrument which oscillates in tune with the applied force takes less power than one which gives a steady deflection.

Again, a moving system of low natural frequency takes less power than one of high frequency, but a search coil operating in a low frequency magnetic field picks up less power than in one of higher frequency.

Taking all these points into consideration, it was decided to use a comparatively high frequency magnetic field, say, 34 periods per second, and to change the frequency of the electromotive force, picked up by the search coil, from 34 periods to one of low frequency (namely, two periods per second), and to apply this to a direct current instrument having the same natural period of oscillation.

A second model instrument was made up by using ordinary commercial type voltmeters. We did away with all gearing and complicated mechanical movements and made the instrument give indications by reflecting spots of light on to a graduated scale from a mirror attached to the voltmeter movement. By this means we were able to measure accurately powers as low as .003 microwatts or 1/100th of the power absorbed in our first model. The instruments were commercial type needing no fine adjustments; they were robust enough to withstand the vibration on an aeroplane and the time lag in operation was negligible.

This was our first real sign of success and was reached in the summer of 1924.

Spurred on by this success we commenced to improve on details, and we now have a third model which is very much smaller and lighter than the second model and will quite easily measure powers of .0005 microwatts or $\frac{1}{6}$th of the power absorbed in our second model and 1/600th of our first model.

On these results it was decided to carry on with a full scale scheme at Farnborough, and we hope very shortly to test instruments of this type on aircraft.

As a result of the various improvements that have been made we shall be able to reduce the current in the ground cable from our original figure of 250 amperes to 50 amperes, with a ground plant of about 3 h.p. per mile instead of 15 h.p. The weight of the search coils carried on the aircraft will not exceed 5 lbs. and we shall be able to obtain indications from at least 1,500 feet downwards.

We have by no means reached the end of our tether, and even now further improvements are in sight which if found successful can be used either for a still further reduction of ground plant or for obtaining indications to a greater height. It is interesting to note that the figure I mentioned above, namely, .0005 microwatts, which is the power absorbed by the instrument to give a deflection of 2 cms. on the scale represents approximately 1/1,500,000,000,000th h.p.

Steering Directions

I will now explain how the indications are obtained to enable the pilot to steer along the magnetic track.

It is well known that the electromotive force induced in a search coil depends on the number of magnetic lines embraced by the coil. The number of lines embraced is proportional to the projected area of the coil when viewed in the direction of the magnetic lines.

Two search coils are used, and these are set at angles such that they embrace equal number of magnetic lines when on the track, but unequal if the aeroplane is off the track or turns to get off the track.

We have two main conditions to deal with when endeavouring to keep the aeroplane on the track—one in which the aeroplane is parallel to the track but to either side of it, and the other when the aeroplane is on the track but turns in a manner which would soon take it off the track. Similarly we have two directions to deal with in considering our magnetic field. If the magnetic field were visible we should see, when looking vertically downwards, streams of

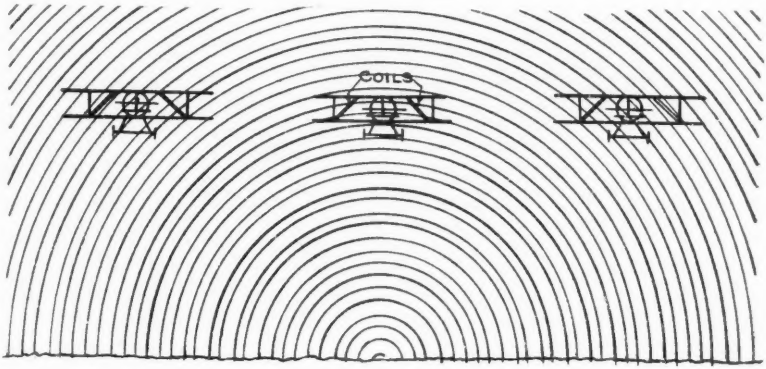


FIG. 1.

parallel lines all at right angles to the ground cable. If we take a vertical section of the space above the cable and at right angles to the cable, we should see the semi-circular arches as already explained.

We will first suppose that two search coils are placed between the wings of a biplane as shown in Fig. 1, each coil sloping at 45 degrees to the horizontal plane. When the aeroplane is on the track where the field is horizontal, each coil presents the same effective area to the magnetic field and equal electromotive forces are induced. If the aeroplane moves to the port side of the track but still parallel to it, the effective area of the starboard coil is increased and that of the port coil is decreased. If the aeroplane is on the starboard side of the track, the effective area of the port coil is increased and that of the starboard coil is decreased. The coil which presents the greater area and so picks up the greater electromotive force indicates on which side lies the correct track.

We will now assume that we have a second pair of coils which are placed vertically between the planes but sloping at 45 degrees to the plane of symmetry as shown in Fig. 2.

If the aeroplane is on the track and flying parallel to it, the port and starboard coils present equal effective areas to the magnetic field. As the aeroplane turns to starboard the effective area of the port coil is increased and that of the starboard coil is decreased. As the aeroplane turns to port the effective area of the starboard coil is increased and that of the port coil is decreased. The coil

which presents the greater area and so picks up the greater electromotive force indicates in which direction to turn to get parallel to the track again.

It will be noted that the effect obtained by the coils arranged as in Fig. 2 is exactly the same as that obtained in the coils arranged as in Fig. 1. That is to say, a transverse movement in one direction has just the same effect as a turning movement towards that direction.

The angles described in Figs. 1 and 2 are combined in a pair of six-sided coils, and the positions of the sides with relation to a cube are shown in Fig. 3. Sides 1 to 6 form the starboard coil and sides *a* to *f* the port coil. In the starboard coil the vertical sides 1 and 4 embrace an area equivalent to a slope

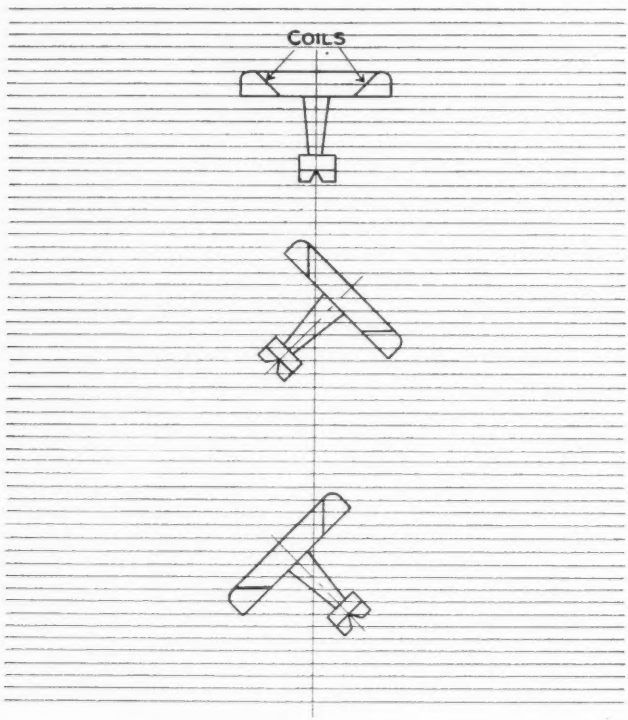


FIG. 2.

of 45 degrees to the plane of symmetry of the aeroplane, and 2 and 5 embrace an area equivalent to the slope at 45 degrees to the horizontal. Similarly in the port coil the vertical sides *a* and *d* are in a plane at 45 degrees to the plane of symmetry and sides *b* and *e* in one at 45 degrees to the horizontal.

The advantages of this shape of search coil are that they can be fitted around a fuselage without interfering with the space inside the fuselage, or they can be fitted in a biplane by embodying 2, 3, *b* and *c* inside the top plane and 5, 6, *f* and *e* in the bottom plane with the vertical sides stretched between the planes or possibly inside or alongside vertical struts.

The Instrument

The instrument is rectangular in shape, the present model being about 11in. high × 7½in. wide × 4½in. deep. The height could have been reduced, but was made of this size to obtain a good open scale.

On the face of the instrument are two frosted glass scales graduated in feet to represent height above ground and a third indicating scale as shown in Fig. 4.

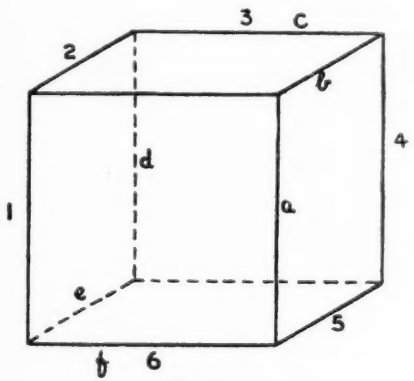


FIG. 3.

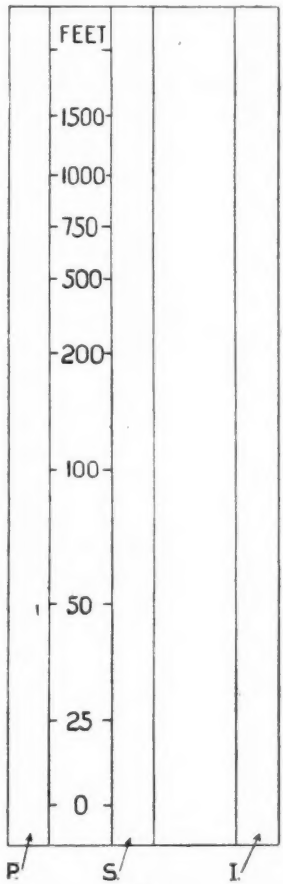


FIG. 4.

Three moving coil instruments are mounted in the bottom of the case and reflect beams of light which appear as spots of light on the scales. As the instruments operate the spots of light move up and down the scale, the amplitudes of movement representing the electromotive force induced in the search coils. The zero position of the instrument is at the top of the scale.

The spots of light on the P and S scales oscillate at two periods per second and their amplitudes of oscillation represent the E.M.F. induced in the port and

starboard search coils respectively. When these two spots of light have equal movements the aeroplane is on its track and height above the ground is recorded.

If the port reading is greater than the starboard, then the pilot should turn to port to regain the track; similarly, if the starboard is the greater, he should turn to starboard.

The third scale 1 has been added to our latest model and is used to indicate to the pilot his position on the track.

I have already mentioned that a pilot must know his position as well as his height when attempting to land, and this is necessary even when the pilot knows he is circling around a definite track. It is practically impossible to arrange a track over ground which would be suitable for the final landing on any point of the track. The track is therefore arranged to contain a straight section which passes over ground suitable for the final landing. The scale 1 indicates to the pilot when he is over this section of the track and also shows him his progress along it.

The spot of light on this scale oscillates at four periods per second and is therefore quite distinct from the other two.

The frequency reducer is a most important piece of apparatus in connection with this scheme, and a brief description of its operation will probably interest you. As already mentioned, this system of measuring alternating currents depends on the reduction of the frequency of the current picked up by the search coil. If a circuit containing an alternating current is reversed at a frequency equal to that of the alternating current, a direct current would be obtained. If it is reversed at a slightly different frequency, the current obtained will slowly alternate at a frequency equal to the difference between the frequency of the alternating current and the frequency of the reversal.

At present our search coils pick up current at 34 periods per second, the frequency reducer reverses the circuit at 32 periods, and the resultant current at two periods per second is applied to the instrument. There is, of course, one pair of reversing contacts for each circuit.

If the reversing contacts in the port and starboard coil circuits operate in unison, then the spots of light on the P and S scales oscillate in unison. If we arrange the circuits so that the spots of light are in step and then reverse one of the coils, the spots of light will be out of step. Here, then, we are able to indicate the relative polarity of two search coils carrying alternating currents. This point is of extreme importance in the operation of the scheme, as will be explained later.

Our latest model frequency reducer has three sets of reversing contacts, one each for the port and starboard search coils operating at 32 per second and one for the indicating search coil operating at 64 periods per second. When the search coils are over the landing section they are in a magnetic field of 34 periods, on which is superimposed another field at 68 periods per second. These superimposed frequencies will be induced in all the search coils, but the frequency reducer sorts it out and serves up the current at two periods for the P and S scales and the current at four periods for the indicating scale without one being in any way affected by the other.

The Scheme in Operation

When your Council honoured me about twelve months ago with an invitation to read this paper, it was hoped that, by now, full scale trials would have been more advanced.

On an investigation of this type it is practically impossible to prophesy date of results and more especially in this case, which involves the laying of cable over a considerable area of land.

During the past year laboratory experiments have continued and these have resulted in improvements in the efficiency of the scheme to such an extent that the ground plant now being installed is only about one-fifth of the size of plant that would have been required if these improvements had not been effected.

In describing the scheme in operation I am only able, at the present time, to give you results based on calculation but checked as far as possible by experiments on a small scale model.

Our small scale model track consists of a cable about 125 feet long laid on the floor of a large shed. The track has two parallel sides joined by semi-circular ends, the distance between the parallel sides being about 20 feet. A small indicating coil is laid alongside one straight side to indicate the section of the track suitable for the final landing. The scale of this model is approximately 1/200th full size.

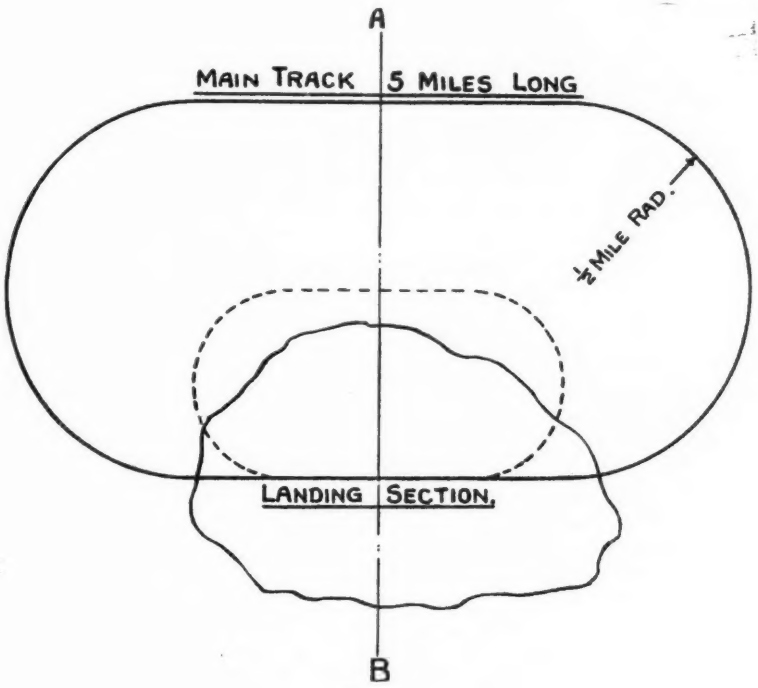


FIG. 5.

The instrument is of course full size, but the search coils are on the same scale as the track, viz., 1/200th full size. The search coils are fitted in a miniature aeroplane which can be manœuvred above the track.

This small model has been extremely useful in the development of the scheme. We have been able to imitate aircraft movements on our miniature aeroplane and to study accurately the effects as indicated on the instrument. Perhaps the most interesting demonstration is one in which one operator is blindfolded and carries the model aeroplane, and a second operator gives him verbal directions by watching the instrument without being able to see the first operator. The instrument is connected to the miniature aeroplane by a long

flexible cable. The blindfolded operator carries the aeroplane above his head representing about 1,200 feet altitude and is led to the vicinity of the track moving at a rate equivalent to 1/200th full scale speed. As soon as the second operator obtains indications on the instrument he shouts out his steering directions and is very soon able to get the first operator travelling around the track. By further instructions as regards height the second operator can make the first operator gradually lower the model aeroplane, which eventually touches the floor on the landing section.

A few minutes ago I described the direction of the magnetic field by telling you what we should see if the magnetic lines were visible. The landing instrument is a means for enabling the pilot to see in effect the shape of the magnetic field through which he is flying, and by this means to recognise his position with respect to the track. I will now explain the condition of the lights on the instrument in various positions above the area affected by the current in the ground cable.

Fig. 5 shows a five-mile track with one side passing across an aerodrome on which is the section for the final landing. The dotted lines represent the indicating loop which energises the indicating movement in the instrument and shows when the aeroplane is passing over ground suitable for final landing.

The instrument gives steering indications to enable the pilot to fly around the main track and simultaneously records the height above ground.

I will first take a sectional view on A B and explain what happens when the aeroplane is travelling parallel to the track and then a plan view showing the effects of turning.

Fig. 6 represents the sectional view drawn approximately to scale.

The dotted lines H represent the positions where the field is horizontal at all heights up to 2,000 feet. It will be seen that for the first 500 feet this line is practically vertical but then bends outwards.

Lines X1, X2, X3 and X4 represent the positions where the field is 45 degrees to the horizontal. The direction of the magnetic field is not semi-circular around the cable, but is distorted outwards due to the effects of the current in the opposite cable.

The correct track for the aeroplane is on the line H, which, it will be noted, forms a track which gradually increases in size as the height is increased.

The shaded V section of the diagram represents the total area of space where the direction of the magnetic field is less than 45 degrees to the horizontal. This V section occurs all around the track.

The arrows at the top of the figure represent the relative amplitude of movement of the P and S spots of light on our instrument when the aeroplane is travelling parallel to the track. Arrows in the same direction represent that the oscillations of the two spots of light are in step, and when the arrows are opposite the oscillations are out of step.

If we are on the correct track H the two spots of light are equal and in step, as shown in positions 4 and 12. If we are on the port side of the track the starboard light remains about the same but the port light is reduced. When we get over to X3 on the port side, the port light has dropped to zero and the starboard light is about the same as when we were at H. If we move still further to port, the port light starts to oscillate again but out of step with the starboard light, as shown in position 9. When we get to the centre of the area enclosed by the track the magnetic lines are vertical and the two lights are equal and out of step as shown at 8.

Positions 1 to 7 represent aeroplanes coming in the opposite direction, and it will be noticed that the same simple rule holds good, namely, that the side

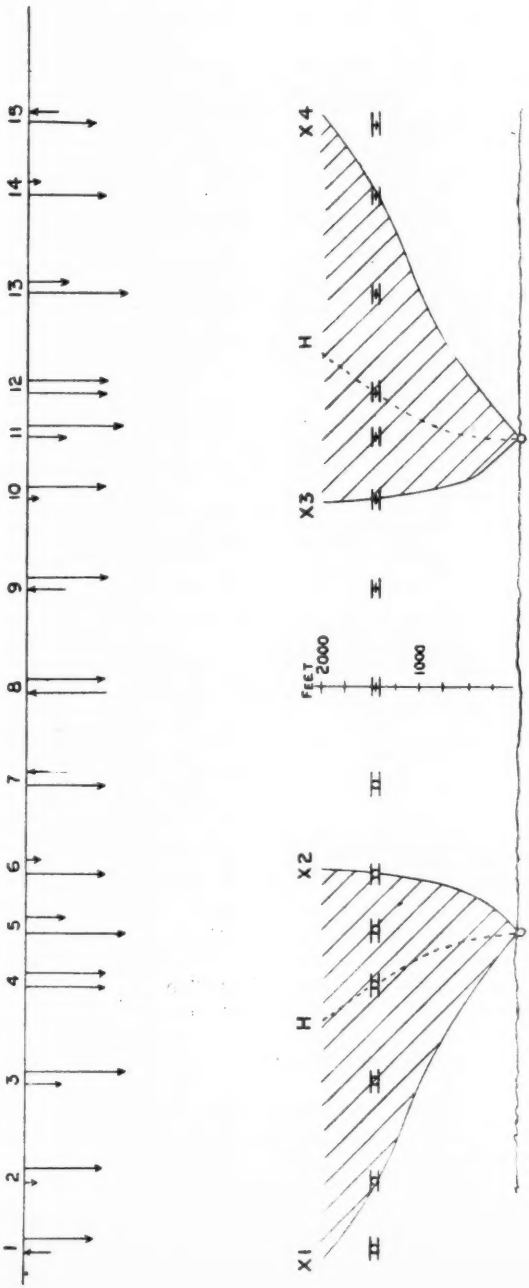


FIG. 6.

which shows the greater amplitude represents the side on which lies the correct track.

It will be noted that when the aeroplane is in a position where the direction of magnetic field is less than 45 degrees to the horizontal the spots of light are in step, and in all positions where it is greater than 45 degrees to the horizontal the spots of light are out of step.

The line of positions represented by H will be called the track, but the area between X1 and X2 or between X3 and X4 will be called the path of the aeroplane. The width of the path is equal to approximately twice the height above ground.

It is not necessary for the aeroplane to keep exactly on the track because a wide path is provided on which it is possible to tell the approximate height. For example, at H the exact height is recorded by the amplitude of movement of both spots of light. At all positions between H and the edge of the path one spot of light gives the height within 20 per cent. On one edge of the path one spot of light drops to zero.

The pilot should endeavour to keep within his path, and this will be made easy because directly he gets into the area between X2 and X3 or beyond X1 or X4 his spots of light start the out-of-step movement, thus giving a very definite indication that the aeroplane is off the path.

With practice the pilot should have a very good idea of his position on the path and could if he so desired make an inner circle route along positions X3 and back along X2, keeping his port reading at zero, or he may take the outer circle route along X4 and back by X1 by keeping his starboard reading at zero.

It will be noted that the path gets narrower as the height is reduced, and for this reason the pilot should not attempt to do the complete circuit at less than, say, 500 feet, where he would have a 1,000-foot path to travel in. The final landing is done along a straight portion of the track, where it will be much easier to keep to a narrow path.

Opinions of pilots differ on the question of how near the ground height indications are required. Some are of the opinion that they will be required until ground is touched, and others say they can land without further indications below 50 feet. This is one of the points which can only be proved by full scale trials, but if it is found that a wider path is required near the ground level, I have no doubt that it can be arranged. In connection with this point I may mention that experiments are also in hand to illuminate the landing section with lines of Neon lights, which can be seen for some considerable distance through fog, and thus give a visual indication of the landing section when near the ground.

Before leaving this sectional view of the landing area I will explain why the two spots of light are out of step when the angle of the magnetic field is greater than 45 degrees to the horizontal.

Fig. 7 shows the two coils sloping at 45 degrees to the horizontal and 90 degrees to each other in a field which for clearness is shown exactly semi-circular. The two faces of each coil are represented by one full line and one dotted line. At all points between positions 1 and 2 where the direction of the magnetic field is less than 45 degrees to the horizontal the lines at any instant enter both coils by the full line face. In position 2 where the field is 45 degrees the port coil embraces no lines. In position 3 where the field is greater than 45 degrees the lines at any instant enter the port coil by the dotted line face and the starboard coil by the full line face. The relative polarity of the two coils has been altered and the lights are out of step as previously explained.

I will now show the effects of turning movements.

Fig. 8 is a plan view of a portion of the track showing the two parallel sides. At the top of the figure I have shown in plan aeroplanes in the same positions as we have just seen in the sectional view. These aeroplanes are parallel to the track, positions 1 to 7 show the aeroplanes travelling along one side in one direction, and positions 8 to 15 show the aeroplane travelling on the other side in the opposite direction.

I want you to imagine that you are travelling in this aeroplane and the arrows in front of the aeroplane will then represent the relative positions of the two spots of light as seen on the instrument.

H represents the correct track, and X1 and X2 represent the edges of one path and X3 and X4 the edges of the other path.

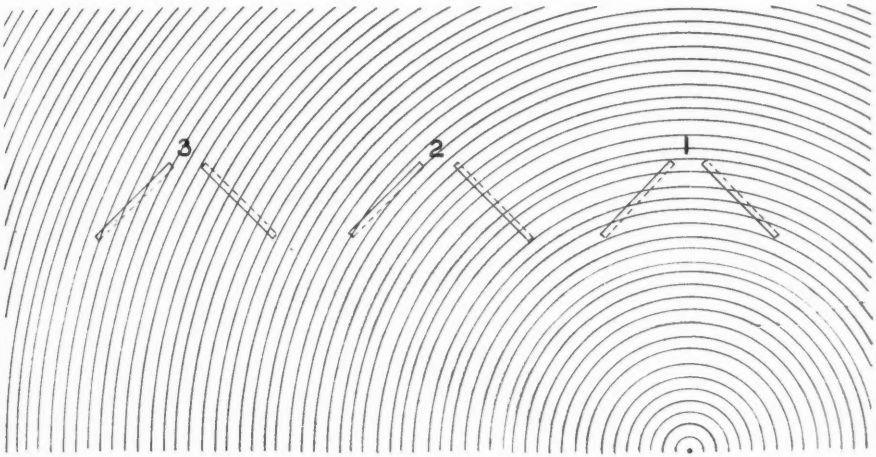


FIG. 7.

As before, it will be noticed that the side which has the greater light amplitude represents the side on which lies the correct track.

The object of the pilot is to get his lights equal and in step.

Now a turn towards the side having the greater amplitude will always bring the lights equal, but the amount of the turn depends on the position of the aeroplane.

If he is a little way from the correct track he has to turn through a small angle; if he is further away he turns through a greater angle. If he is right on the edge of the path he has to turn 90 degrees before he can get the lights equal.

I have shown these turns on the figure in position 1A, 2A, 3A, etc. Now the effect of this gradually increasing angle of turn to equalise as the aeroplane is travelling across the path results in a gradual curve of positions leading on to the correct track as shown on the figure.

There is only one position where the aeroplane can keep his lights equal and in step, and that is on the correct track. He may be going in either direction, but he must follow this track if he keeps his lights equal and in step.

When the aeroplane is on the path there are two curves of positions where the lights are equal and in step—one sweeps on to the track in one direction

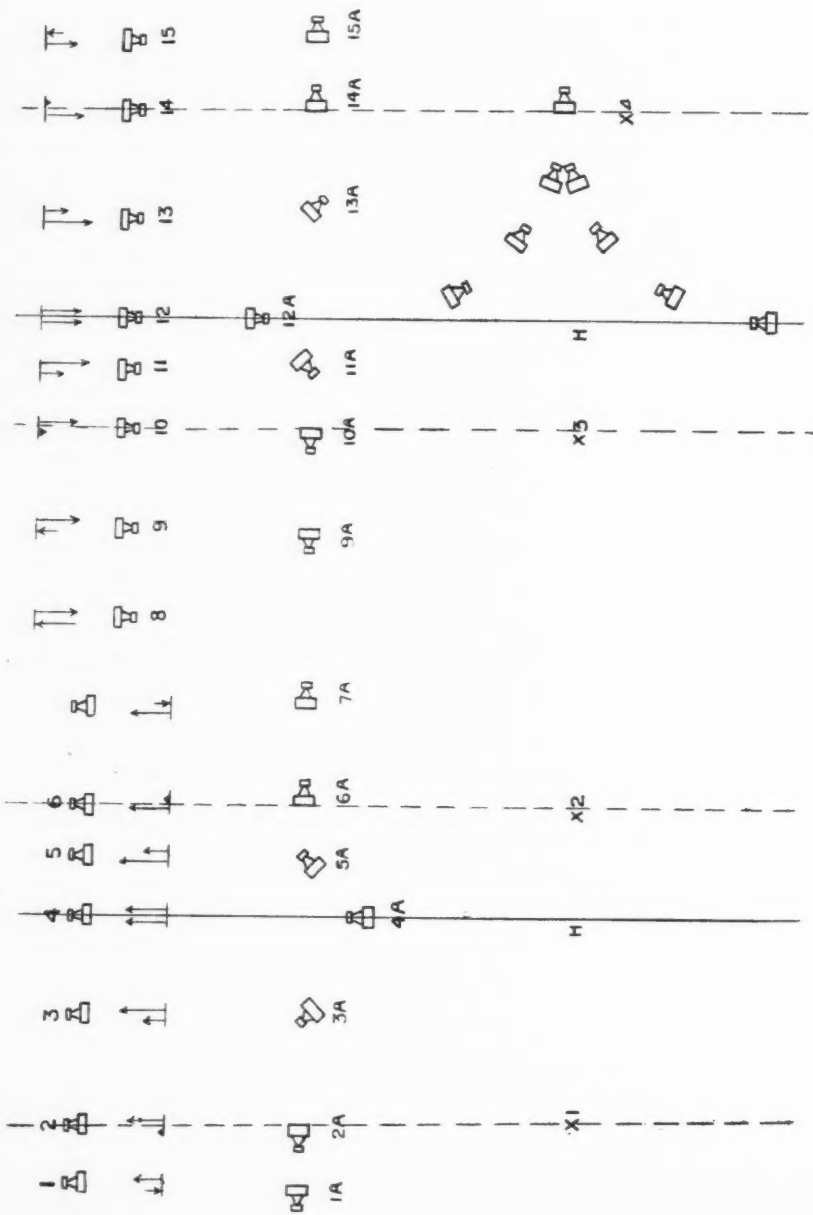

15
15A
14
14A
X10
13
13A
12
12A
H
11
11A
10
10A
X5
9
9A
8
7
7A
6
6A
X12
5
5A
4
4A
H
3
3A
2
2A
X1
1
1A

FIG. 8.

and the other in the opposite direction. When the aeroplane reaches the edge of the path at right angles he can follow either of these paths. If he deviates slightly to port he would sweep round in a curve to port and get on the track in one direction. If he had deviated slightly to starboard when coming on to the path, he would have swept round a curve to starboard and got on the track travelling in the opposite direction. Whichever path he takes he is soon brought on to the track and cannot leave it if he follows the steering directions.

When the aeroplane is in the centre of the area or in any other position off the path, the lights are out of step. But the pilot still turns in the correct direction to equalise, and the only direction he can follow by steering to the indication is one which leads directly towards the path at right angles. If for example he was travelling directly away from the track when he first took notice of the steering indications, these indications would bring him right round until he was flying directly towards the path.

There is no possible wrong steering direction, and if the simple steering rule is followed the aeroplane must take the direct route to the track and then stick on it. In fact, the instrument is almost human in giving steering directions.

You will naturally want to know what happens during banking, climbing and diving.

If an aeroplane is banking the correct track will not appear to be where the magnetic lines are horizontal but where they are at an angle equivalent to the angle of bank.

If an aeroplane has a bank of 22½ degrees with port wing down, the track will appear to be about half-way between the correct track and the edge of the path on the port side.

A bank with port wing down will decrease the starboard readings. Now a bank to port is nearly always accompanied by a turn to port, and a turn to port when on the track will increase the starboard reading. These two actions, which usually take place simultaneously, cancel each other as regards their effect on the instrument reading.

An aeroplane banking when going around the semi-circular ends of the track will try to follow a track which has a radius slightly less than that of the correct track.

Climbing and diving while on the track do not affect the instrument readings in any way because these movements are around an axis which is parallel to the direction of the magnetic lines and the effective areas of the search coils are thus unaltered.

Fig. 9 is a plan view of the whole track, the full lines representing the large main cable track and the small indicating loop, drawn approximately to scale. The length between 15 and 16 is the section suitable for the final landing.

Dotted line H represents the correct track where the magnetic lines are horizontal and lines X3 and X4 represent the two edges of the path where the magnetic field is 45 degrees to the horizontal. These are also drawn to scale and represent the width of path at 1,500 feet altitude.

Position 1 shows an aeroplane approaching the area, and 2, 3, 4, 5 and 6 give the approximate path when following the steering directions on the instrument.

Position 7 shows an aeroplane heading away from the area, and 8, 9, 10 and 11 indicate the probable path when following steering directions.

An aeroplane must get well on the track or path at 1,000 to 1,500 feet and should circle round the track before attempting to descend.

After doing a complete circuit at, say, 1,000 feet, the aeroplane can gradually

FIG. 8.

descend to 500 feet by keeping the lights of the instrument equal and reading off the height indicated.

Positions 12 to 18 indicate an aeroplane circling round at 500 feet just previous to landing. You will notice that at this height his track is in a position almost vertically above the ground cable.

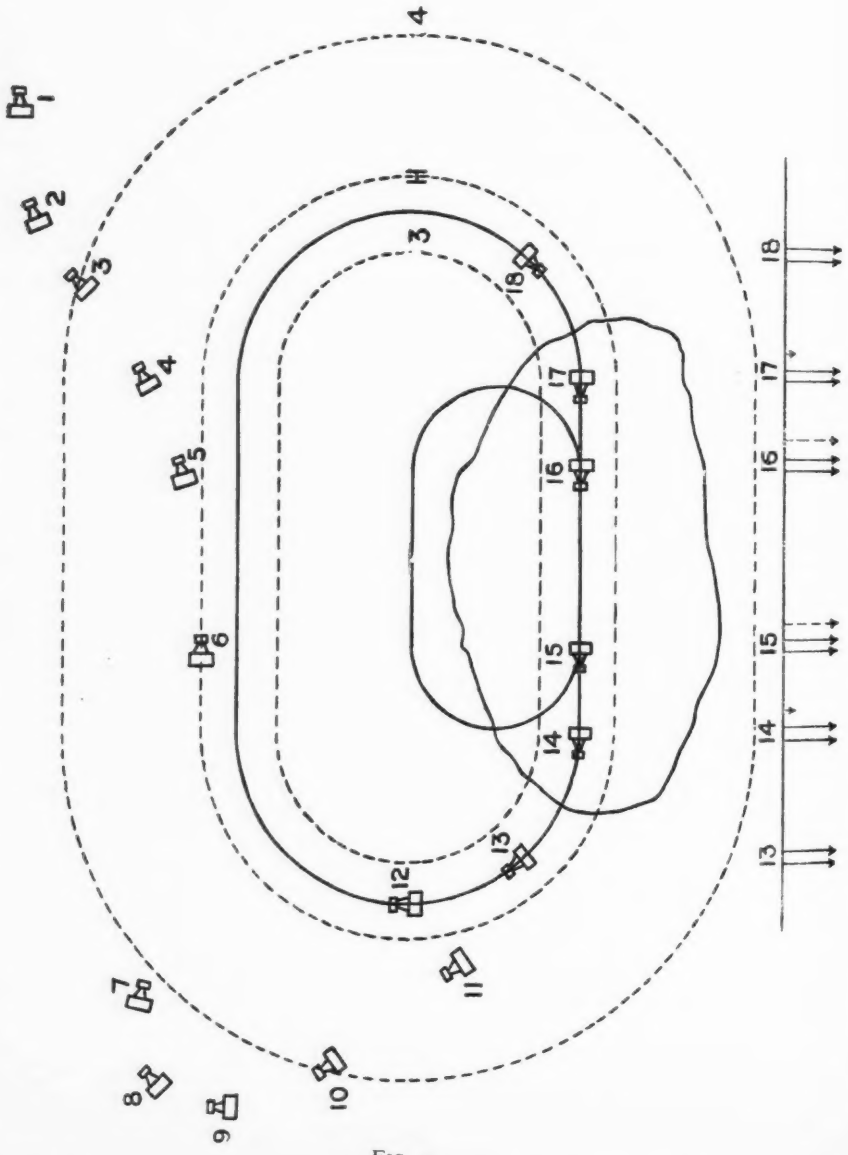


FIG. 9.

The arrows at the bottom of the figure indicate the relative positions of the lights on the instrument. At 14 the indicating light commences to record as shown by the small arrow on the right. At 15 the indicating arrow has increased and remains equal to the two height indicating arrows until 16 is reached. Here the indicating light starts to recede and reaches zero at 17.

The pilot now knows that he has passed over the landing section at 500 feet. He does another circuit at this height and directly the indicating light starts to record again he will start his glide to land. As he descends the two main indicators will increase in amplitude and record his alteration in altitude.

The indicating light will keep level with the other two all the time while travelling between 15 and 16, which is the section suitable for landing, and as long as the pilot lands before the indicating light starts to recede, he will know that he is on this section.

If for any reason the landing is not completed before the indicating light starts to recede, the pilot will elevate and do another circuit.

Conclusions

There are many interesting points I have not described in detail because they would have involved much technical detail on electricity and magnetism, but if there are any points I have not made clear, I hope you will let me know and I will do my best to explain them.

In working on a scheme of this description there are many auxiliary problems which are not so spectacular in the solution but which none the less had to be solved before the scheme could be made a success. One of these problems is the question of keeping constant speed on our ground plant and on the frequency reducing apparatus carried on the aircraft. This and other auxiliary problems have, as far as can be foreseen, been completely solved, but the work on them has taken up a very considerable proportion of our time.

The method of measuring alternating currents which I have described may in the future play a very important part in connection with aerial navigation. When it is remembered that the power which can be accurately measured is far less than the power required to give an audible signal in a telephone receiver on aircraft, its possibilities in connection with aerial navigation can hardly be foreseen.

As aerial transport increases, as it most assuredly will do, so will the need increase for a means of directing our aircraft into well-defined routes.

For centuries past the earth's magnetic field has been a means for navigating ships at sea, and so in the future magnetic fields may be increasingly used for navigating aircraft. The earth's magnetic field is a stationary one and fixed as regards directions. The magnetic fields of the future will be alternating and directed in a number of required directions.

The guiding of aircraft, and probably ships at sea, may eventually be done by these alternating magnetic fields, the high frequency ones being used for directing craft over long distances, and a scheme, such as I have described, for bringing the craft into port.

As ships at sea are guided over long distances by the help of the compass and the earth's magnetic field and are brought into port by a pilot and a line of buoys, so in the future may aircraft be guided over long distances by directional wireless and brought into port by definite tracks on which height can be accurately recorded.

Although these controlled magnetic fields will no doubt gradually replace the earth's magnetic field in usefulness, it is my personal opinion that we might make better use of the earth's magnetic field. For example, the instrument I have described could, with the addition of a small auxiliary, be used on aircraft to steer any desired course by utilising the earth's magnetic field. By setting the required course on a dial the instrument would give steering directions to keep to that course just in the same way as when following our cable course. The slightest deviation from the course would be definitely and instantaneously recorded.

I am also of the opinion that a means could be found for utilising the earth's magnetic field in connection with the experimental measurement of turning and other aircraft movements.

The scheme I have described is not yet complete and difficulties may occur when we start our full scale trials, but I am confident that they will be overcome. Other means may be found for measuring the strength and indicating the direction of the magnetic field, but there is no doubt that we have in the magnetic field a means whereby aeroplanes can be guided and landed in fog.

Discussion

The Chairman said: I am very sorry Sir Sefton Brancker is not here to-night as he looks upon this problem as one of extreme importance. He regards fog as the greatest remaining difficulty in maintaining the regular working of commercial aviation. R.T. promises to give a very satisfactory system for guiding aeroplanes to the aerodrome, and the system which has just been described is a very promising method of enabling aeroplanes to land.

It has not yet been tested on full-scale, and we must admire the courage with which the Author has agreed to come before a critical audience before his method has been tried out on full-scale. We may therefore give him a thoroughly critical discussion of his paper so as to bring out the points which are in doubt. This system will assuredly be tried on full-scale. Each critic has therefore a chance of distinction if he finds the troubles which do actually develop under full-scale tests.

Mr. Handley Page said: The Chairman has rather intimated that anyone who takes part in the discussion must be a prophet. Far from wishing to be considered a prophet, I appear in the *rôle* of a seeker of information on this very interesting subject.

I quite agree with what Sir Sefton Brancker has said with regard to fog. Fog is the greatest bugbear that commercial aviation has to deal with—fog and bad visibility generally. If we could get over that difficulty we could fly all the year round instead of during the summer months only. But I think the instruments will have to wait a long time before we can convince the average pilot that two spots of light moving on a scale can guide him to safety. In a bad fog, what he would like would be a frosted glass screen on the dashboard of the cockpit which would show him exactly where the ground was if he could see it. If the Lecturer could substitute for the two spots of light a complete picture of the aerodrome, what an improvement it would be!

One question that occurs to me is the possible distortion of the magnetic field when the aircraft is flying through it. If the aircraft is of metal it will have induced currents which would distort the field, and I wonder if the Lecturer sees any difficulty there? In any district where there are a number of power cables they might lead to other distorting effects.

I should also be interested to know why the Lecturer has chosen a 32 period. I believe the Electricity Commissioners are about to standardise on 50 periods. I think he should fall in with the recommendations of the Commissioners.

I am not quite clear on the question of banking. You could fly with one wing up and one down, yet the instrument would give you the same readings as if you were flying properly, providing your wings were tangential to the line of force of the field. If you were a thousand feet horizontally away from the cable, just above the ground, flying with your wing tip vertically below you, you would have the same reading as if you were flying vertically over the cable at a height of a thousand feet.

Captain ACLAND: From the very clear and able paper which Flight Lieutenant Cooch has read to us to-day one is left with a feeling of great confidence that the result of his and other officers' research and experiment leaves very little to enable the system to become successful in a practical way.

I discussed this system with a highly technical electrician and he put forward the following points as perhaps of interest for the purpose of the discussion.

He stated that apparently the period of the exciting current is changed by a mechanical system of vibrating contacts. Has the idea of hetrodyning the period by means of a locally produced alternating current of the correct period superimposed on the top of the picked up current to produce a beat note of the correct period been tried? This eliminates all mechanical effort other than that required to produce the local alternating current.

He also pointed out that the system evolved is remarkable for the response of the instrument to minute quantities of electrical power, and this fact may have a disadvantage in that it may pick up sources of power other than intended and might therefore be misleading. For instance, what would be the effect of a machine travelling within 1,000 feet of an overhead tramline of which the period for which the instrument is set has either the fundamental or the harmonic of the power supplied to the tramway? And the same question would arise in connection with electric tramways and power distributing plants.

The effect would be quite distinctive when once the machine was inside the track, but might be difficult to differentiate when outside.

I have also had the opportunity of mentioning this system to one or two pilots, and there is no doubt that the efforts being made in this direction are bound to be a very great contribution to the solution of the greatest difficulty that exists in organising air routes, namely, landing in fog or mist. This, coupled with the successful experiments which one hears are taking place with the Neon system of lights, will give added confidence to the pilots.

Mr. H. E. WIMPERIS said: Some 18 months ago I had an opportunity of seeing at work the models to which Flight Lieutenant Cooch has referred. The demonstration showed the method to have promise of success, and everyone hoped that full-scale trials would take place at no distant date. Unfortunately, the making of a five-mile trench for the cable takes time, and even yet we are not ready for a full-scale test. Then there was the question where to put the test cable, at Croydon or at Farnborough? If the full-scale test failed to confirm the expectations which had been raised, Croydon would not want it. If, however, success was met with, the apparatus would be needed at the Farnborough aerodrome as well as elsewhere. It was therefore decided to lay it at Farnborough, and it will not be long now, I hope, before the full-scale trials can be begun.

I admire the energy with which the Lecturer has gone about this work and I wish him every success in his further endeavours.

Dr. DRYSDALE said: I have the pleasure of coming here this evening for the first time. Mr. F. E. Smith, the Director of Scientific Research at the Admiralty, had intended to be present himself, but was unfortunately prevented from doing so, and suggested that I should come in his place.

In the first place I should like to congratulate the Author most heartily on this paper and on the valuable and ingenious device he has brought out. I am quite sure he has gone a long way towards distinct success and I sincerely hope that the full-scale trials will justify his anticipations.

The Admiralty have been at work on this subject since 1918 when it was found that the Germans had been using some form of leader cable for directing their course through certain channels, and it became obvious that a leader cable system might be of considerable importance. The system was developed to a considerable extent, and since the war an 18-mile cable has been in use for directing ships into Portsmouth Harbour, but the final perfecting of the system has been dropped owing to the reluctance of commercial shipping companies to undertake the cost of laying and maintaining the necessary cables. One of the serious difficulties in connection with submarine leader cables is that they are liable to be damaged just at the critical moment when they are required, as on the advent of fog, ships are liable to let down their anchors and drag across it. To obviate this, it is necessary to armour the cable very heavily, which not only greatly increases the cost but results in a very large proportion of the return current coming back by the sheath, to such an extent that perhaps only 5 per cent. of the working current is effective for the production of signals. In addition, the high conductivity of sea water produces a further reduction of the signal strength and a serious distortion of the magnetic field from the distribution in circles round the cable, as ordinarily assumed, so that although it has been perfectly feasible to produce a satisfactory aural system of detecting the position of the cable by the assistance of the screening action of the ship's hull, steering indications cannot easily be obtained by using inclined coils on the lines suggested by Lieut. Cooch. As a matter of fact, a visual system using alternating current relays and inclined coils in the rigging of the ship was put forward at the end of 1918, and immediately gave satisfactory results as regards sensitivity and visual indications of the presence of the cable; but distortion of the field caused ambiguous steering indications to be given, and it required fully two years of hard work to get over this difficulty, and even then with only moderate success. In fact, it was necessary to restrict the zone of steering indication to 70 yards on either side of the cable in order to avoid this ambiguity. Fortunately, Lieut. Cooch has not anything like the same difficulties to cope with, with coils on land, but the possibility of distortion owing to the conductivity of the soil must not be overlooked, as if the resistance of the soil is 100 times that of sea water, the same distortion will be produced at a distance of $\sqrt{100}$ or 10 times the distance, which is not great when we consider the considerable heights and speeds of aircraft.

I do not think we have any data as to the resistivity of the earth at moderate depths so that it is impossible to give any accurate forecast as to the amount of distortion which will be produced, but I fear that some of Lieut. Cooch's diagrams will need modification, and sincerely hope that it will not be to such an extent as to give any serious trouble.

As regards the indicating mechanism, the tuned alternating current relays which are used for my own visual system were found sufficiently sensitive for the restricted ranges which were required, and would serve to light a lamp half a mile away from a cable carrying 10 amperes at 15 pps. in about 100ft. depth of water; but I have since produced a very simple form of tuned vibration galvanometer which is probably quite as sensitive as Lieut. Cooch's device and has the advan-

tage of requiring no commutator mechanism and no electrical supply other than that of an illuminating lamp, the indications being given by two streaks of light.

I think that this device is well worth consideration where the greatest possible lightness and simplicity is required. On the other hand, Lieut. Cooch's device has one great advantage in that it indicates reversal of phase which would, of course, occur in the vibration galvanometer device, but would not be noticed owing to the rapid movement of the light. I am very glad in any case, to see that Lieut. Cooch has come to the same conclusion as I did, that visual indications are essential for really effective use of a leader system; and I feel fairly confident that when the leader system is revived for navigational purposes, as it most certainly ought to be, especially in foggy areas like the banks of Newfoundland, some type of visual system will eventually be used.

I would conclude by again congratulating Lieut. Cooch upon his admirable devices and with the sincere hope that they will realise all the expectations he has formed of them, as there can be no doubt that in combination with some luminous indication when approaching the ground they will do an enormous amount to decrease the risk of aerial navigation and make air services much more regular.

Captain F. L. BARNARD: I trust that any criticism I may make hereafter will be regarded as constructive and not destructive. Few people can be in a better position to appreciate the urgent necessity of a satisfactory fog landing device than the modern air line pilot. Personally, I am convinced that it is useless to expect him to continue to take heavily-laden commercial aircraft through worse and worse conditions of visibility year after year unless a considerable change in the present method of procedure is adopted. I am equally convinced, however, that it only needs the commercialisation of various devices *already in existence* to enable him to leave to schedule with a visibility of five yards, fly to his destination 200 or 300 miles away with dense ground fog the whole way, and finally to land in complete safety without being able to see the ground at all. Aircraft often leave Croydon nowadays when the aerodrome and much of the route is fogbound, but it is essential that a reasonable visibility is available at the other end. Developments necessary to overcome such difficulties as engine failure, compass errors, failure of the human element's sense of stability when blinded, instability of certain aircraft and their controls, night and coastal distortion of wireless waves, are outside this discussion, but it is desirable to realise the probable limits of accuracy of wireless directional control instruments which obviously must be developed in conjunction with the aircraft leader cable. What with the distortion already referred to and the possibility of jamming, it would be unwise to count on normally getting nearer than 3,000 or 4,000 yards to the terminal aerodrome by means of wireless beacons. As the machine is probably coming in over the fog, it may be anything up to, say, 6,000ft. high. As there must obviously be a reasonable overlap of the devices, I should say that a range of detection by the cable instrument of at least 8,000 to 10,000 yards is highly desirable. As in order to keep operational expenses as low as possible, the tendency is to keep the cable amperage down, it would seem that the addition of valve amplifiers to the instrument is an obvious development. The question of the weight of the instrument itself is of secondary importance, as if by its means the machine can operate, say, another forty days per annum, much more will have been gained on the swings than lost on the odd roundabout.

I am of the opinion that away from the actual aerodrome itself it might be found advantageous to lay the cable on telegraph poles rather than on the ground.

A point that I am not quite clear about is the effect on the reading of the instrument of a conglomeration of lines of force at any particular point due to

a sudden bend upwards of the cable. Might this not be used with advantage to warn pilots of their approach to a mountain?

With reference to pilots who feel that provided that they are brought over the aerodrome at 50ft. or so in a dense fog they are confident of their ability to land safely, to my mind adherents to this theory will tend to diminish in numbers in proportion to the measure in which they make full scale tests. Time spent in experimental work on their behalf (otherwise than by raising a suitable fund) would therefore be wholly wasted. What is required is undoubtedly an accurate reading to within a foot or so (if possible even less) when close to the ground. It might be possible to attain this end, whilst keeping the instrument within reasonable dimensions. by making a section of the front quarter-elliptic.

I would now like to turn to what I consider the crux of the whole matter—the most advantageous application of the device. It is not at all unlikely that the contrivance will be made or damned by the final solution to this secondary problem. I regret that I think that the suggested lay-out is likely to lead the pilot into innumerable difficulties. It should not be forgotten that a commercial pilot in flight rarely has less than twenty things constantly requiring attention; thus any new control which requires extreme concentration on one point alone and/or much calculation and estimation is definitely dangerous in that other perhaps vital points have to be neglected. In deciding the general arrangement of the cable with reference to the aerodrome it is essential to bear the following points in mind:—

(1) So far as possible the best lay-out should be agreed upon and this made universal so that a pilot landing at a strange aerodrome in fog will not be called upon to do unusual things.

(2) The best landing direction on most terminal aerodromes rarely offers more than 1,000-1,200 yards' run.

(3) Two to three hundred yards may be lost due to the aircraft having to clear obstructions at a safe height.

(4) Landings will probably be made fast rather than slow for fear of getting too close to the stalling point.

(5) If the machine hits the ground rather hard and bounces, the pilot may be compelled to ease the second touch by opening up the engines momentarily.

(6) Items (4) and (5) may mean that the "run" may be anything up to, say, 600 yards.

(7) Disaster would probably result due to the pilot stalling the machine if he instinctively felt that there was a boundary fence not far ahead; in other words, if he rightly or wrongly lacked confidence.

(8) Allow another 300 yards for "confidence."

If there is one thing that is surely very obvious from the foregoing, it is that the pilot must be led easily, without great concentration, and quite slowly, towards the point at which he may touch ground. It probably would be fatal to suddenly inform him "Now you are right over it, land as soon as possible." On the contrary, he should be warned, step by step, "You are now over section 'R'" (for example), which he knows is 400 yards long and the fourth section of, say, 400 yards from the final one; "now passing on to section 'S,'" "now 'T,'" and so on. He will thus know exactly where he is, and his experience will tell him, "You should be not more than 600ft. over section 'R,' 450ft. over 'S,' 300ft. over 'T,'" etc., so that when he does finally come to the landing point, nothing will have to be done hurriedly and he will *slowly* so have adjusted his height on the previous sections that all he has to do is throttle down completely and flatten out normally according to the indicated height on his

instrument. So that machines approaching from any direction are equally treated and cannot possibly arrive unexpectedly right over the aerodrome. I think that the slow spiral would probably be the best shape, though the final three sections *at least* should be quite straight so that the added complication of turning as well as losing height should be avoided when near the ground. Finally, if the landing stretch can be arranged to be slightly uphill (to reduce landing run) so much the better.

I quite appreciate that the dividing of the cable into sections is going to present added difficulties, but it is nevertheless far preferable that difficulties should be experienced on the ground beforehand than in the air subsequently. It might well be that the acoustical-wireless arrangement at present being experimented with at Biggin Hill might be of some use here.

Mr. F. Tymms: It is intended, if the full scale experiments at Farnborough are successful, to instal a leader cable at Croydon, and in conjunction with it a system of Neon lights, which recent experiments have shown to have very remarkable fog-penetrating qualities. Briefly, the arrangement of the Neon lights proposed is a series of lines of Neon tubes radiating from the centre of the aerodrome towards the eight cardinal and quadrantal points. The lines of Neon tubes will not be continuous but divided into illuminated and dark sections. In order to indicate the direction of landing, at the end of each line will be a "T" piece and it will be arranged that any one of these "T" pieces can be switched on, or is automatically switched on by a wind vane, in order to indicate the direction of the wind, or the direction of landing in fog, when there is no wind.

Undoubtedly when the Neon light scheme and the leader cable scheme are combined on the same aerodrome, one line of Neon lights will be laid parallel to the leader cable. Arising out of this, one wonders what will be the effect of the Neon light circuit on the magnetic field from the leader cable?

In addition to the possible causes of distortion mentioned by other speakers, one would like to know what will be the effect on the magnetic field in the vicinity of the searcher coils due to electric circuits, generators, etc., in the aircraft itself?

The only steering direction which the lecturer has not dealt with is the sense of the circuit, that is, whether right-handed or left-handed. It will be advisable for all machines to fly in the same direction, even though, of course, the aerodrome control will only allow one machine on the circuit at one time. This may mean a machine reversing its direction of circuit after starting.

The lecturer's peeps into the future are interesting, particularly the reference to the possibility of guiding aircraft at a distance by means of high frequency alternating magnetic fields. But surely, on his own showing, the power required for distant operation will be very high. For this reason I am more interested in his proposal to make use of the more intensive earth's magnetic field. Do I understand that this can be done with stationary coils of the type developed for this apparatus? Presumably not, since to obtain an alternating E.M.F. in the stationary earth's magnetic field, rotation of the coils must be introduced. If this is so, what improvements over the American earth inductor compass does the lecturer visualise?

The reference to the guiding of aircraft by means of directional wireless as ships at sea are guided by the help of the compass shows, I submit, an incorrect association of ideas. There is a common tendency to regard D/F W/T as something which has taken the place of navigation. It has done nothing of the kind; it is merely an additional instrument in the hands of the navigator, and moreover, not one which does the job of the compass. It is a means of fixing position. At sea it is taking the place of the Sextant, the Lead and the Pelorus. In the air it is taking the place of the pilot's view of the ground. Both the ship and the aircraft still require the compass to steer by as much as ever. There may some

day be invented a wireless compass, but it does not exist; it will not exist until the selectivity of wireless sets is such that it will be possible to keep a station, or rather a number of stations, transmitting unceasingly for aircraft to steer on without any danger of jamming anyone else. When that has been done we can get down to designing a radiogoniometer which will allow the pilot to keep the head of his craft at a definite angle to the wave front of the transmission. Since this angle changes with the position of the craft, except in the simple case of steering towards the transmitting station, the task is not easy.

In the meantime, the pilot must steer by his compass between transmissions, and in the meantime also we shall have the earth inductor compass, which Flight Lieutenant Cooch has promised us—to my mind, a far more practical thing.

Mr. TOULMIN SMITH said: I took part in some experiments we carried out in connection with Neon lights at Croydon. They consisted of Neon tubes laid on the ground, the object being to find what effect these tubes gave when observed from above through fog or mist, observations being taken from a captive balloon. The results were very promising, and certainly I thought that if used in connection with the leader cable system they would take away a lot of the anxiety which pilots may have during the final landing. We were able to see the tubes quite clearly through thick fog 1,200 feet deep and having a ground visibility of about thirty yards. Another attribute which will be of great advantage is the very pronounced red glow which is seen both through the fog and above the fog. This glow indicates the position of the tubes, and can be seen when approaching in fog at approximately twice the range of the tubes, so that a pilot following round a leader cable having tubes placed parallel to the landing portion of the cable should know when he was above that portion by means of the visual indication given by the red light within fairly wide ranges, and on descending could follow the tubes and land by them.

This experiment was made at night, but we were also fortunate enough to be able to do some ascents during daylight in fog of practically the same density, and even then we saw the light from the tubes at 150 feet. Even in daylight the tubes can give an indication which should relieve the pilot of a great deal of his anxiety.

Mr. ERNEST T. WILLIAMS said: I think there are a few points which might usefully be discussed.

It is noted that Flight Lieutenant Cooch is proposing to use lights for indicating a landing ground as an auxiliary to his electro-magnetic system. I think this is very wise. In connection with some trials for night landing arrangements for which I was responsible in 1917, in addition to their being useful for indicating the landing position, they were found to be most valuable for the guidance of pilots flying in fog or ground mist. One day the Commander went off before dark, when it was quite clear, but a fog came on later, and he could see nothing. It occurred to the officer of the station that it might be helpful if the night landing lights were switched on, and this was done. The Commander saw what he later described as "a bank of white wool," and, realising that it was probably the landing lights shining through the fog, he attempted and made a good landing. He always said that this had saved his life. I therefore think that the association of some form of landing light with the leader cable system is very important.

I am not quite sure how far Flight Lieutenant Cooch has satisfied himself that there will be interference with the leader cable system through electric railways and tramways. In connection with the Admiralty Observatory at Greenwich, it was found that unless certain precautions were taken, the interference was very pronounced, and later, when it was decided to electrify the South-Eastern

Railway, it was thought to be more economical to build a new magnetic laboratory outside the region of interference, rather than take the elaborate precautions which would have been necessary on the railway itself. I suggest that the effect of railway electrification on the leader cable system requires very careful consideration.

I do not know whether Flight Lieutenant Cooch contemplates laying armoured cables for the leader cable system. In some leader cable experiments at sea with which I have been associated it was found that the effect was greatly increased if cab-tyre sheathed cables were used instead of armoured cables. They suffer from the disadvantage, however, of being easily damaged.

In 1917 the importance of landing in relation to the direction of the wind was always emphasised. In our night landing experiments we used to try to indicate to the pilot the direction of the wind. From the paper to-night it would appear that the pilot will be expected to land in any direction relative to the wind.

I would like sincerely to congratulate the author on his paper, and on the system which has been evolved.

Lieutenant-Colonel H. P. T. LEFROY: The results to be obtained from the apparatus of which we have heard this evening may give a partial solution of the general problem of knowing, on an aircraft, one's exact position relative to other material objects nearby. In daylight, when there is no fog, and on clear moonlight nights, a satisfactory solution of this problem is given by normal eyesight, but when the normal rays on which human vision depends are absorbed by a relatively opaque medium, such as fog, then it becomes necessary to use apparatus for the solution of this problem.

A satisfactory solution should give a pilot :—

1. His position in the vertical plane, *i.e.*, his height above the ground.
2. His position in the horizontal plane, relative to the landing ground.
3. His position relative to obstacles in front of him, such as mountains, cliffs and icebergs.
4. His position relative to other aircraft that may be near him when he is about to land.

In addition to :—

(*a*) The R.A.E. method, described by the author, there are two other possible methods worth considering, namely :

(*b*) Infra-red ray method ;

(*c*) Acoustic echo method.

With reference to the above four desiderata :—

(*a*) should give (1) and (2),

(*b*) should give (2) and (4), and a visual estimate of (1) and, possibly, (3) by reflection.

(*c*) should give (1) and (3), and, possibly, (4).

I have always felt that the final solution of this problem, to be aimed at, should be one which allows the pilot to use his normal vision, because he could then use his sub-conscious faculties in estimating his proximity to the ground, etc., which seems to me of supreme importance ; for this reason I have sought for a solution of the problem by method (*b*). It is well-known that a thick fog is almost transparent in infra-red rays of suitable wavelength. It is also known that a photograph of a given piece of country can be obtained by means of infra-red rays only. If the incidence of infra-red rays on a screen of some material could cause that material to give out visible rays proportional to the intensity of the infra-red rays, then, if flood lighting by infra-red rays, and concentrated beams of the same rays, could be arranged on a landing ground, it should be possible to obtain a picture of that

landing ground on a suitable screen on an aircraft; such a screen should, preferably, be arranged on special goggles. At present, generators of infra-red rays are very inefficient, and detectors of such rays are very insensitive, so that it has not yet been possible to design any practical apparatus for seeing through fog, but this may become possible by future developments. A previous speaker has pointed out that excellent results have been obtained at Croydon, by the use of Neon glow-tubes, for seeing through fog; their efficiency for this purpose is due to their being near the red end of the spectrum, and for longer wave-lengths, such as I have suggested above, fog would be still more transparent.

I consider that the best way to approach a landing ground is by the use of rotary radio beacons; there should be no errors with them, such as occur at sunset, etc., with the Bellini-Tosi system. An ordinary receiving set and a stop watch are the only apparatus required on the aircraft.

To sum up, it seems that, at the present time, the best combination for landing in fog can be obtained by:—

(i.) Approaching the landing ground by radio direction-finding.

(ii.) Descending through the fog by apparatus such as has been described this evening.

(iii.) Landing by visual observation of Neon glow-tubes.

Flight Lieutenant COOCH in his reply said: Mr. Handley Page asked about distortion due to metal aircraft. We have tried the effect of metal in various ways in our small-scale experiments by imitating metal engine parts and metal wings, and the greatest distortion we could obtain was one causing a reduction of 3 per cent. on the electromotive force induced in the search coil. This is a small error, and can be corrected by adding 3 per cent. to the number of turns on our search coil. A frequency of about 32 periods per second was chosen simply because it was the frequency of the existing plant used on our experiments. Later on we may increase this frequency, because an increase in frequency will mean a reduction in size of ground plant, but at the same time it will mean that greater accuracy in speed will be necessary on our frequency reducing apparatus. With regard to "banking," Mr. Handley Page has visualised the interesting condition of an aeroplane keeping parallel with the track, but altering its position and bank simultaneously, so that the wings of the aeroplane are always tangential to the magnetic field. This, I think, would be quite impossible in practice.

At this stage of the investigation it is not claimed that the scheme will give indications to keep the aeroplane trimmed laterally, but the turning movement to avoid side slipping during a bank does correct the variations on the instrument due to the bank. In any case, if flying in foggy weather is to become general, the aeroplane will have to be fitted with a bank indicator or automatic aileron control for operation at times other than when attempting to land.

The question of heterodyning as mentioned by Captain Acland has been considered, but abandoned firstly because the production of an alternating current of a definite frequency on the aircraft would be more difficult than the provision of reversing contacts, and secondly, because an alternating current thus superimposed will not change the frequency of the induced currents picked up by the search coil in such a manner that they will affect a direct-current instrument. Captain Acland and others asked about interference from tramways and other sources of electrical power. In all commercial systems of electric power the two cables forming the circuit run near together, and the external magnetic field produced is very small indeed. An overhead tramline will have a return circuit in the rails say 20ft. below, and the external field from these two conductors at 1,000ft. altitude would be only about 2 per cent. of that produced by the one conductor if the other is removed to a distance similar to that used in our track.

It must be remembered that the instrument and frequency reducer form a mechanically tuned system which will only respond to magnetic fields of a definite frequency. A different frequency will not affect it, as instanced by the fact that we use a current at 68 periods on our indicating ground coil, the magnetic field of which does not in any way affect the steering and height-indicating instruments, which are tuned for a magnetic field of 34 periods.

Dr. Drysdale has very kindly pointed out some of the difficulties experienced in connection with the leader cable system at sea, and more especially with reference to distortion of the magnetic field. In my opinion the distortion he has described is almost entirely due to what is known as "earth return," that is to say, the return circuit for the current is through the medium surrounding the cable. In our scheme we make the current follow a definite path and the return circuit is through the cable on the other side of the track about a mile away. It is true there may be some distortion due to eddy currents in the soil, but at the low frequency we are using the distortion should be extremely small. We have examined the shape of the magnetic field of a small cable loop buried on Farnborough aerodrome, and no distortion could be detected at frequencies up to 600 periods, even when the ground was so saturated that water stood in pools above the area covered by the cable loop.

I was interested in the galvonometer described by Dr. Drysdale, which apparently works at a comparatively high frequency.

The possibility of being able to distinguish relative polarity of the search coils, will, I feel sure, prove one of the most important features of the scheme when we come to the full-scale trials, and at this stage it would not be advisable to forgo this advantage to use high frequency galvanometers. There is no doubt that an instrument of greater efficiency could be designed, but up to the present we have been content to use commercial apparatus.

Captain F. Barnard has mentioned valves. I have tried to make use of them, but up to the present their usefulness is problematic. If they are used as amplifiers we could increase the height at which indications are received, reduce the weight of the search coil or the size of the ground plant. The weight of search coils is now about 5lbs., so any reduction due to valves will be partly, if not entirely, cancelled by the weight of the valves with their batteries, etc. The size of the ground plant is about 3 h.p. per mile, and will probably be less in the near future. Any increase in height, if required, can be obtained by increasing the power of the ground plant, and this appears to be far preferable to installing valves with every instrument.

The introduction of valves for operation with the instrument would necessitate a certain amount of adjustment, and cause irregularities, and this would be contrary to one of our main objects, which was to obtain an instrument needing no adjustment.

As regards the difficulty in finding the area embraced by the ground coil, this, of course, can be overcome by increasing the area of the loop. 15 h.p. for a five-mile track is not a large plant, and the track could be increased to 10 or even 50 miles without necessitating an excessive plant. The cable at Farnborough will probably be partly run on poles.

The instrument measures height above the cable, and if the cable follows the contour of the land, the height above land is recorded.

Where the ground starts to rise the density of the magnetic field is increased and the instrument will therefore anticipate the rise. Similarly, if the ground starts to dip, the magnetic field is less dense, and the instrument anticipates the dip. The percentage difference in these two cases would, however, be small for ordinary changes of contour.

There would be some difficulty in arranging the instrument scale in the form suggested.

As regards indicating the position on the loop, the system is extremely flexible, and can be adjusted to suit requirements. All that is necessary is to decide what is the best heights and positions to approach a particular aerodrome for landing, and to lay the track accordingly. The indicating device can be made to start recording at any position, say half-mile or one mile from the aerodrome, and the relative positions of the indicating column with respect to the height indication will show how much of the half-mile or one mile has been traversed. The indicating column is equal to the height columns only when over a position where final landing is possible.

An alternating magnetic field of this low frequency cannot be affected by absence of daylight, but if the cable is laid under sea there will be some distortion of the field, and also some irregularity due to the contour of the sea bed.

In reply to Mr. F. Tymms, the direct current circuits and generators on the aircraft will not affect the instrument, because it is a definite mechanically tuned system. The Neon light circuits, even if fed with A.C. of the same frequency as the ground cable, cannot affect the scheme, firstly because the current is, I believe, under half ampere, and scondly, because the return circuit is placed alongside the tubes, and the external field is therefore negligible.

With regard to my "peeps into the future," they must of course be regarded only as "peeps," but in my opinion the present instrument if successful as a means for landing could be made a two-purpose instrument by making it suitable for following a compass course. The auxiliary would contain rotating coils with an attachment to produce alternating current at two periods per second which would be applied to the present instrument. The oscillating instrument has an efficiency of 30 to 50 times greater than a steady reading instrument, and the advantages thus obtained could be utilised either for a reduction in the size of the rotating coil or in an increase in indications obtained per degree deviation from correct course.

My suggestion of using high frequency fields for steering over long distances was in reference to beam or directional wireless stations at definite important points with apparatus to steer along the beam. With regard to this point I would refer Mr. Tymms to the remarks of Major Binyon in his inaugural address before the wireless section of the Institute of Electrical Engineers ("Journal of the I.E.E.," Vol. 64, No. 348, December, 1925).

I should like to thank all for their interest and more especially for the very useful criticisms which, in my opinion, have fully justified the bringing forward of the paper before full-scale trials have taken place.

Wing Commander T. R. CAVE-BROWNE-CAVE, in proposing a vote of thanks, said: There is no question as to the importance of this system to aeroplanes, and this paper has been of enormous interest. The admirable way in which the Author has tackled the problem and the ingenuity of his devices, particularly the frequency reducer, are very evident. I am sure that all those present will wish him every success in his full-scale trials.

Contributed by Lieut.-Comdr. G. McAlpine.

As the individual who initiated the attack on this problem in 1920, I would like to record my appreciation of the way in which the Author has stuck to the task and surmounted each obstacle as it arose. I might say that the original conception was to get right away from the apparently unavoidable disability attaching to the "leader cable" by virtue of its return current, via armouring

and sea water, by the use of some form of closed cable loop. The first loop experimented with was of circular form, and efforts were made to measure the vertical component of the magnetic field as the aircraft crossed the central enclosed area. It soon became evident that existing types of A.C. instruments were hopelessly inadequate to the task, and efforts were made to rectify the A.C. current and use standard types of D.C. instruments. Any scheme of this character, however, by which an area only is marked, requires an auxiliary device to indicate continuously the centre of the landing area, and efforts in this direction were made towards the production of a type of "dipping" magnetic needle which pointed always to the centre of the area and incidentally gave a secondary indication of altitude. At this stage Lieut. Cooch took up the problem, and I think all electrical engineers will agree that the methods of solution adopted for each difficulty, as it arose, constitute an object lesson in the clean application of established engineering principles to what is essentially an *ad hoc* piece of development work. The superlatively high efficiencies obtained in the novel type of oscillating electric motor developed—for the indicating instrument is nothing less—are due to the precise way in which the classic principles of electric motor design have been followed. It is unfortunate that the limitations of time and money have prevented a full-scale trial being made ere now, but this delay has at least had the good effect of enabling the aircraft end of the scheme to be developed to a higher degree of efficiency.

Contributed by D. E. Woods Humphery.

It is with the greatest regret that I find myself unable to attend Flight-Lieutenant Cooch's paper to-night.

It is not very often that I can summon up enough courage to assist in the debates which follow these papers, but in this case I feel impelled to do so, because this paper is so outstanding both in the able way in which it is presented by Flight-Lieutenant Cooch, and in the extreme importance of the subject to civil aviation.

First of all I should like to congratulate Flight-Lieutenant Cooch on the modest and clear way in which he has carried us through the history of several years of research work, which required great determination and tenacity of purpose, and which must have met during its progress with many disappointments.

I am afraid that hitherto I have underestimated the value of the Electrical Department of the Air Ministry, but after reading this paper I am converted. At the same time I would commend the Department to concentrate its energy on work of this kind and leave the design, manufacture and selection of ordinary common electrical equipment for use on aircraft to the common engineer, like ourselves.

My association and experience in electrical work is now I am afraid so antiquated that I feel unable to make any comments on the technical side of Flight-Lieutenant Cooch's most valuable paper, but I do express the sincere hope that sufficient encouragement and money may be available to enable the Electrical Department to continue this most valuable research, and I hope that we may be furnished from time to time with further information on this subject, either by Flight-Lieutenant Cooch or another member of the Electrical Research Department.

NOTES ON STALLED FLYING

BY C. HOWARTH, ASSOCIATE FELLOW.

This paper deals with experiments carried out on Avro 2402, subsequent to the report T.1757 of October, 1922, on Stalled Flying, by Squadron Leader R. M. Hill and H. L. Stevens. (This report will shortly be published as R. & M. 963).

Previous to this full-scale tests had extended at most to angles of incidence just beyond the stalling angle and any data used in calculations involving greater incidences than these had to be taken from model tests, thus possibly introducing errors due to scale effect. It is therefore very important to obtain, in the case of at least one standard aeroplane, a full-scale determination of the total lift and drag to as great an angle of incidence as possible.

The total lift and drag of an aeroplane can be obtained from the following observations taken during a glide:—

The forward velocity (Vi), the aneroid height and temperature each minute, from which the rate of descent (Ve) and the density of the air (σ) can be obtained, the angle of some fixed part of the aeroplane (usually the lower chord at root) to the horizontal, $(\alpha - \gamma)$, measured by means of an adjustable bubble inclinometer, the total weight of the aeroplane during the glide (W). A knowledge of the wing area is also required.

The paper will be divided in four parts:—

(a) A description of the development of the experiment.
(b) The method of reduction of the observations.
(c) The important points noticed during the experiments.
(d) An appendix.

(a) *A Description of the Development of the Experiment*

This describes the attempt to obtain the total lift and drag of the aeroplane, in steady flight, to as large an angle of incidence as possible ($+30°$), by means of glides with the engine switched off.

The aeroplane is a standard type Avro 504 K, except that a fin and large rudder were fitted to improve the control at large angles of incidence. At the same time, the undercarriage was replaced by one of the Vee type.

To enable the pilot to hold the aeroplane at large angles of incidence, a ballast tank, fitted with an emergency release, was placed in the rear bay of the fuselage, connected with a tank in the observer's cockpit, from which a known quantity of water was run back, prior to the commencement of a glide.

The combination of the swivelling pitot head and suspended static head, having been found to give an accurate determination of speed, up to the stalling angle (Report R. & M. 854.* See also appendix), it was decided to use this combination during the experiment, on the assumption that it would still be reliable beyond the stalling angle.

On the first attempt considerable height was lost in getting the water ballast back to the tank in the rear of the fuselage. The aeroplane was held at as large an angle of incidence as was possible, with the engine full on, and during this operation was very unsteady. From the commencement of the glide violent longitudinal and lateral oscillations were set up, similar to those described in the earlier report. The needle of the airspeed indicator in the observer's cockpit

* R. & M. 854. "The Measurement of Aeroplane Speed, with Special Reference to the Use of the Suspended Static Head." H. L. Stevens.

was swinging through 20-30 m.p.h. when the suspended static head was observed swinging from side to side and rising above the level of the aeroplane; the emergency release was at once pulled and the pilot regained control. The suspended static head was then observed lodged on the elevators, but was pulled clear in a subsequent dive. Thus it was considered impracticable to use the suspended static head during stalled glides.

The next step was to endeavour to obtain the speed course factor for the standard pitot static system, at large angles of incidence, by carrying out a series of stalled flights with the engine full out, using the suspended static head to obtain the accurate indicated speed. These flights were very unsteady, in fact on one occasion the suspended static head was swinging so violently that it jumped out of the clips which fixed it to the aeroplane and was lost overboard. It is interesting to note that the largest height drop obtained in the series with the engine full out was of the order of 1,900 feet per minute.

A few points were obtained for a speed course factor curve, but owing to the conditions under which they were obtained, were not very reliable. The stalled glides were then re-commenced, using the standard pitot static system. As the height to which the Avro could be induced to climb was very limited (owing to the fin, large rudder and ballast tank, this aeroplane was much heavier than a standard Avro), and the rate of descent at large angles of incidence so great, there was not time for many readings of airspeed and inclinometer; four or five of each, at the most, were all that could be obtained during any one glide. Owing to the oscillations set up during the glide, the speed varied from 8-10 m.p.h.; thus with so few spot readings the accuracy of these results was not high. To overcome the difficulty due to the varying speed, an air log was fitted on to the port strut in the same relative position as the pitot head on the starboard side. The air log measures the distance the aeroplane travels in a definite time, and thus the mean speed for that distance can be obtained. (See appendix for description of this instrument.)

A series of glides was carried out over a large range of incidences. using the air log to determine the speed. It was found, however, that when using the speed course factor obtained from the " engine on " flights, the total lift coefficient was gradually increasing beyond the stalling angle, contrary to the results obtained from model tests. This indicated an apparent error in the speed course factor. At this stage in the experiment another pilot took over the work, and on his first flight at approximately 26° incidence, the aeroplane was remarkably steady, there being no trace of the old oscillations; the variation in speed was of the order of 2-3 m.p.h., while the inclinometer bubble was almost stationary. (His method of getting into the stalled state was different from that of the previous pilots. This will be referred to later.) Having achieved steady flight in a stalled state, it was decided to revert to the suspended static head again, to obtain the correct indicated speed and thus be freed from the necessity of obtaining a speed course correction factor. The suspended static head was used for a series of stalled glides, and was observed during one of them, both by the observer in the Avro and by an observer in another aeroplane, and was seen to be very steady. The results obtained from this series are the subject of a report, T. 2067, on the Full-Scale Determination of the Lift and Drag of an Avro, Type 504 K., at Large Angle of Incidence, by C. Howarth.

(*b*) *The Method of Reduction of the Observations*

The method of reduction of the observations is similar to that described in R. & M. 859.* From the observations, the following data are obtained:— The mean height, speed, air density, inclinometer reading and height drop pe-

* R. & M. 859. " Lift and Drag of the Bristol Fighter with the Wings of Three Aspect Ratios." R. A. E.

minute. The weight of the aeroplane and the quantity of petrol and oil used during the experiment are known. The weight of the pilot and observer, suspended static head, and any other extra load carried is added to the weight of the aeroplane, thus giving the total weight of the aeroplane at the commencement of the glide.

The equations of motion are as follows:—

(1) $k_E \rho_o S V i^2 =$ total weight $\cos \gamma$.

(2) $(k_D + k_R)\, \rho_o S V i^2 =$ total weight $\sin \gamma$.

From (1) and (2) $(k_D + k_R)/k_E = \tan \gamma$.

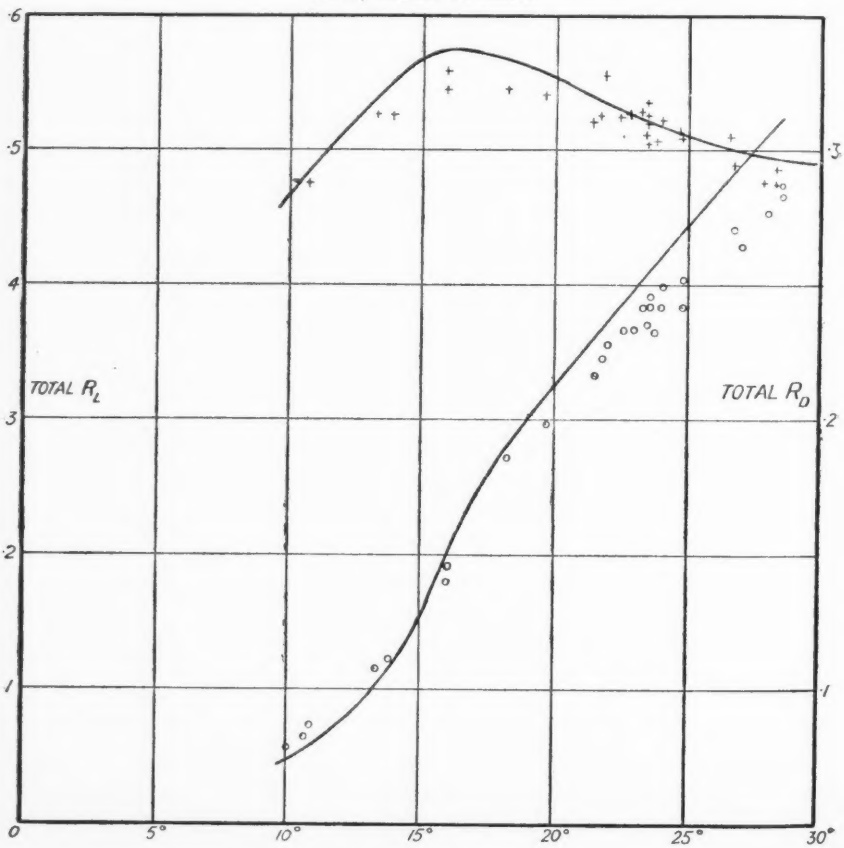
It follows that if Vi is constant throughout the glide, α and γ must also be constant. Thus Vc must be proportional to $1/\sqrt{\sigma}$ and the curve of the glide, which consists of the height plotted against time, is concave upwards. For the

FIG. 1.

TOTAL LIFT AND DRAG CURVES OF AVRO 2402.

Points, Full-Scale Results.

Line, Model Results.



height range usually used the concavity is small, and it is sufficiently accurate to take the mean value of angle of glide, density, indicated speed and inclinometer to give a single determination of k_E (or total k_L), $k_D + k_R$ (or total k_D) and α.

A correction was applied to the measured airspeed for lag in the suspended static system, which is important owing to the high rate of descent at large angles of incidence. The correction amounted in the worst case to about 1.2 m.p.h. It was determined experimentally by subjecting the system to similar conditions on the bench. (See appendix for experimental determination of this correction.)

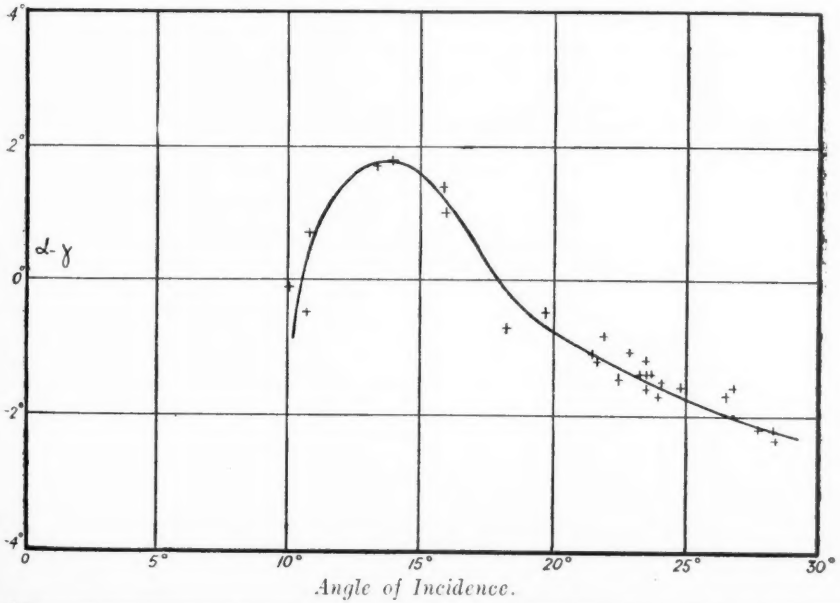
A standard centre of gravity ($x = 1.49$ feet behind the lower leading edge) was taken and in cases when the actual centre of gravity was different from the standard position the difference in tail lift was calculated. This correction is applied to k_L as explained in R. & M. 897.* The tail incidence not being known, the corresponding correction to the drag curve has not been applied. During the glides readings were taken of an airspeed indicator connected to the swivelling pitot and suspended static system, also one connected to the standard pitot static system; and assuming the former to give an accurate determination of speed beyond the stalling angle (see appendix) the speed course correction factor for the standard system was obtained.

The accuracy of the results for incidence above stalling is quite high; on a good glide the total variation in speed is from 2-3 m.p.h. Only one glide in twelve was abandoned owing to oscillations being set up by bumps or otherwise. The values of total $k_L + k_D$ are plotted against incidence (Fig. 1) and also the values of $\alpha - \gamma$, *i.e.*, the angle of the wing chord to the horizontal plane (Fig. 2). The speed course correction factors obtained at various angles of incidence are

FIG. 2.

($\alpha - \gamma$) OR ANGLE BETWEEN WING CHORD AND HORIZON WHEN GLIDING.

Full-Scale Results.

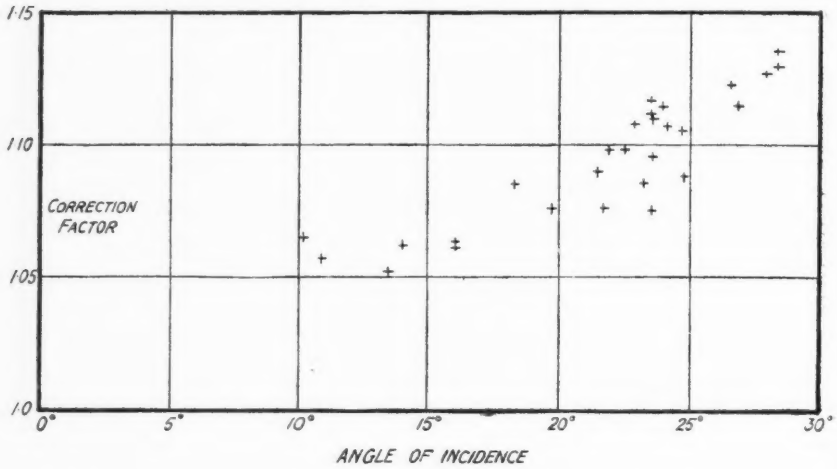


* R. & M. 897. "Lift and Drag of a Standard Bristol Fighter." R. A. E.

given in Fig. 3. A model of the Avro was tested in the wind channel, and there is good agreement between the full-scale and model results.

FIG. 3.

CORRECTION FACTORS FOR STANDARD PITOT-STATIC SYSTEM DEDUCED FROM SUSPENDED STATIC AND SWIVELLING PITOT HEAD.



It is interesting to note that the greatest height drop recorded during a stalled glide in this series was 2,200 feet per minute at 29° incidence.

(c) *The Important Points Noticed During the Experiment*

In the earlier work it was decided that the pilot should hold the stick hard back and control the aeroplane by means of the rudder and the ailerons. This method was first adopted for running the water back into the rear ballast tank; the aeroplane, meanwhile, was very unsteady and lost considerable height during the process. This also meant that as soon as the observer signalled that the water was all back and the pilot switched off the engine, the aeroplane was in a most unstable condition to commence the glide. Under these starting conditions it was never possible to get steady, and a so-called "stalled glide" consisting of a series of violent oscillations, similar to those described in the earlier report, was the result.

It was then found possible to run the water back to the rear tank without stalling the aeroplane, hence a much steadier condition prevailed than before, also height was gained during this period, which was very important owing to the low ceiling of the aeroplane. The importance of being in a steady condition at the commencement of the glide then became apparent, for it was found that if a good beginning was made it was comparatively easy to maintain a stalled state. It was very important that no violent motion of the aeroplane should be made as the engine was switched off. The only successful way of becoming stalled in a steady condition was to endeavour to get the aeroplane into the correct attitude for the glide before switching off the engine; and if this was accomplished, the glide was invariably a success. When a large amount of water ballast was run back, the pilot gradually eased the control column forward to keep the nose of the aeroplane from suddenly rising, and endeavoured to keep the aeroplane on a steady straight course with the use of the rudder and the

ailerons (with much ballast the stick was against the dashboard by the time all the water was back). As soon as the water was back the pilot switched off the engine and kept the stick against the dashboard, allowing the aeroplane to stall gently. The stick was kept forward until sufficient speed in the subsequent dive was attained to make the elevator control effective, then the stick was very gradually pulled right back and the aeroplane usually settled down to a steady incidence. If, however, it was not caught on the first stall, the stick was immediately pushed right forward and the above method repeated. Having once commenced the glide in a steady state, it continued so, unless the weather was bumpy or the pilot over-corrected a slight disturbance. Practically the whole of the successful glides in the series using the suspended static head were carried out by Flying Officer H. J. T. Saint, whose method of control was quite different from that described in the earlier report, for, after the commencement of a glide, he scarcely used the rudder, and when so, very gently. This in all probability was due to the care he exercised in commencing the glide, which he developed into a fine art, for only one glide out of thirty was abandoned due to a faulty start. His method of correcting a slight disturbance which resulted in a dropping wing, was first to push the stick in the direction of the falling wing and then suddenly swing it right across the cockpit and continue this action by a series of rapid jerks until the wing was level again. (It should be noted that on an Avro about 80 per cent. of the total aileron angle can be obtained with the control stick right back.) He also found that slightly easing the control stick forward helped considerably in restoring control when the aeroplane was disturbed during a glide; this is quite the opposite to the statement made by Squadron Leader R. M. Hill in the earlier report, who remarks that easing the stick tends to make matters worse; this is probably due to the fact that he never had the aeroplane in a steady state during the glide. The experiment has proved that with practice and reasonable conditions, steady stalled glides are possible up to at least 30° incidence.

APPENDIX.

Swivelling Pitot and Suspended Static Heads

Bernoulli's theorem states that $p + 1/2\rho V^2$ is constant along a streamline, provided energy is not dissipated due to eddies; hence, if the pitot tube is along the direction of the streamline, the presence of disturbance due to wing interference does not affect this value. To obtain the speed of the aeroplane, which is given by the pressure, $1/2\rho V^2$, it is necessary to obtain the normal static pressure. If there is no interference present the static tube gives this pressure, therefore if the static and pitot tubes are connected to an airspeed indicator or velometer the correct indicated airspeed will be shown. If, however, there is interference in the neighbourhood of the static tube, the local static pressure will be obtained, and it is then necessary to calibrate the aeroplane over a measured distance to obtain a correction factor which will allow for this error. To obviate the time and difficulty of obtaining this correction factor for all aeroplanes used on full-scale experimental work, experiments were carried out to find the best position to place the static tube, so as to be free of interference; and it was found that by suspending the static tube some 30 to 40 feet below the aeroplane, this was accomplished.

The error due to the pitot tube being placed at a small angle to the direction of the streamline is negligible (10° deflection gives an error of 1 per cent.). When flying at large angles of incidence, with a fixed pitot tube, however, the error due to the streamlines cutting across the pitot tube at a large oblique angle is important; it is therefore necessary to allow the pitot tube to swivel.

The combination of a swivelling pitot head and a suspended static head has been thoroughly tested and found to give accurate results up to the stalling angle, but no absolute method has been used for checking whether this combination is

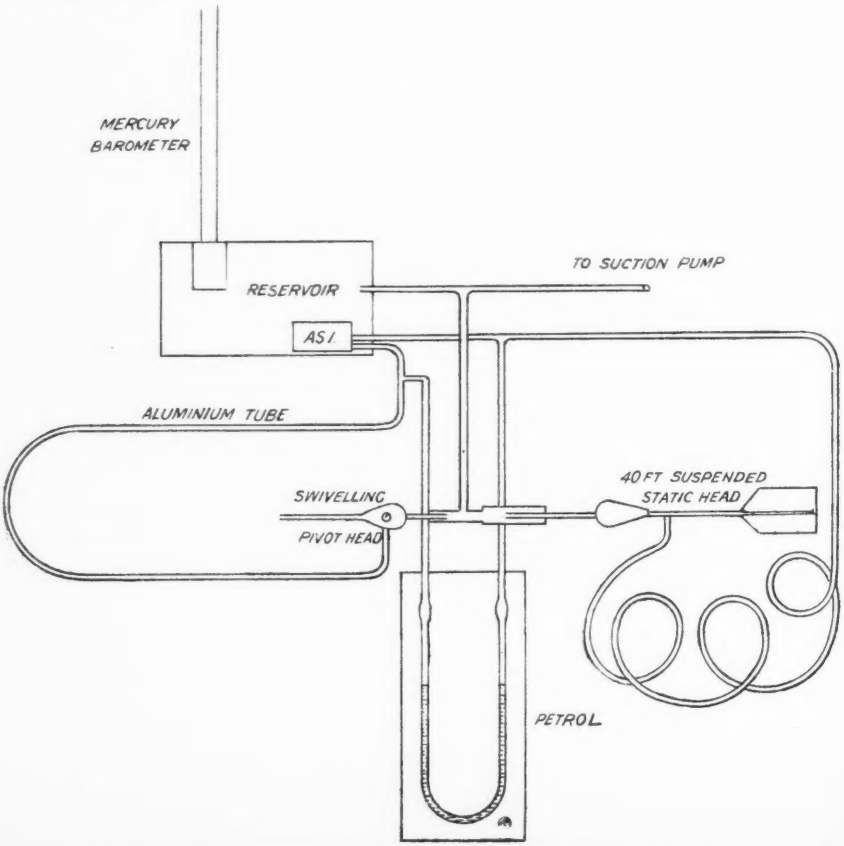
still reliable above the stalling angle, but it is interesting to note that two different types of suspended static heads have been used which, when corrected for lag in the system, show close agreement.

Further, as a relative check on whether interference was present at this distance from the aeroplane, a stalled glide was carried out with the suspended static head 32 feet out for the first half of the glide and full out (40 feet) for the remainder of the glide, and the readings compared with the standard fixed pitot static system. No change was observed in the correction deduced for the standard system, and it is therefore concluded that there is no interference present below 32 feet.*

It is worthy of note that correction factors for the standard pitot static system of the order of 15 per cent. are indicated at 25° to 30° incidence.

Measurement of Lag in the Suspended Static System

The tests for lag in the system were carried out as follows:—The aluminium tubing from the swivelling pitot head to the airspeed indicator, the swivelling pitot head, the airspeed indicator and the suspended static head used during the experiments, were removed from the aeroplane to the laboratory, where the tests were to be made. The following sketch shows a diagrammatic representation of the experiment.



* These tests were carried out on the Avro during the present experiments.

In the experiment, the pressure was varied between the pressure at ground level and that at 8,000 ft., measured by means of a mercury barometer. This pressure was alternately increased or decreased, while readings of the level of the petrol in the U-tube were taken at every 1000 ft. between 2000 and 6000 ft. The time taken to increase or decrease the pressure was also noted, and the equivalent rate of change of height per minute was plotted against the lag in mm. of petrol.

It was necessary to place the airspeed indicator in the reservoir of varying pressure owing to a leak in the static side, when the pressure was below atmospheric pressure, introduced through the joint made between the glass and the case.

It is of the utmost importance that there be no leak in the pitot static system. Both the swivelling pitot tube, the suspended static head and the airspeed indicator should be tested both before and after each flight.

To Convert the Lag in mm. of Petrol to m.p.h.

The s.g. of the petrol at 16° was 0.738.

$$V \text{ ft./sec.} = 66.2\sqrt{h} \text{ where } h \text{ is in inches of water.}$$

$$V^2 \text{ m.p.h.} = (4382 + 60^2 \times .738\,h)/(25.4 \times 88^2) \text{ where } h \text{ is in mm. of petrol.}$$

$$V^2 = 59.2\,h$$

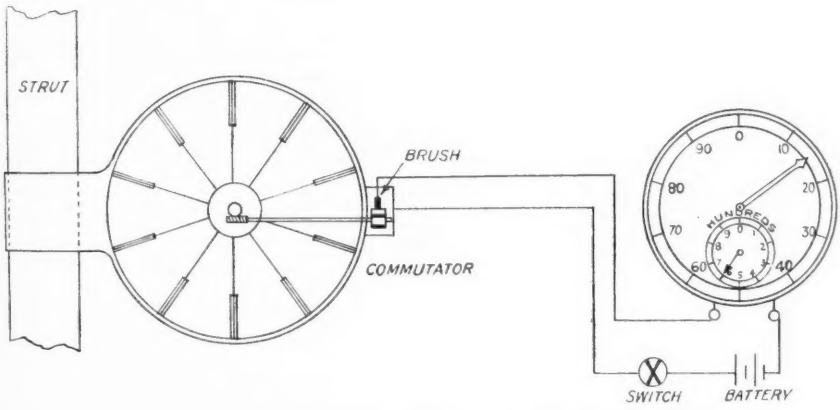
$$V = 7.70\sqrt{h}$$

Let V_1 be speed in m.p.h. corr. cal.
V_2 be speed in m.p.h. cor. cal. and lag.
x be lag in mm. of petrol.

$$\text{Then } V_2 = (V_1^2 \pm 59.2\,x)^{1/2}$$
$$\text{or } V_2 = V_1 \pm 29.6\,x/Vi.$$

The negative sign is used for a glide.

Description of the Air Log



The air log consists of a small windmill mounted on the strut and a recording mechanism in the observer's cockpit. As the spindle of the windmill rotates, it revolves a commutator through a worm gearing; wires are carried from the commutator to the recording mechanism through a switch and a battery. As contacts are made between the brush and the commutator, the electric current operates a trip gear in the recording mechanism and this revolves the hand of

the indicator; a counter-gear is also provided showing the number of revolutions of the hand; the dial is graduated into 100 divisions.

In practice the number on the counter-gear and the position of the hand are noted. When the aeroplane commences a manœuvre the electric circuit is closed and the stop-watch started simultaneously; at the finish of the manœuvre the circuit is broken and the stop-watch is stopped—the number on the counter-gear and the position of the hand are again noted. The difference in readings and the time taken being known, the average speed of the aeroplane can be obtained from a wind channel calibration of the instrument. This instrument measures the true speed of the aeroplane and to convert to $1/2\rho V^2$, the indicated speed as given by the airspeed indicator, it is necessary to know the temperature and density over the range used.

REVIEWS

Skyways

By Alan J. Cobham. Nisbet and Co. 15/- net.

This is a book of sky endeavour and of modest courage. It is a book which should be read by all who have the skyways at heart, who believe in the future of the air.

I have rarely read a book which has amused and thrilled me so much. Alan Cobham makes light of the most harassing of difficulties, skims as lightly as air over the dangers which he faced in his fight for fame. In 1919 the author of this book tramped round to every aeroplane company begging for a job. Five years later he had become one of the best known pilots in the world, a pilot who is known personally on most of the aerodromes of Europe and many of those in Asia and Africa. That in itself is a record of endeavour which is well worth reading.

Skyways is full of good things, from the offer of free flights to the first two girls to kiss a certain bashful petrol agent at Abingdon to the story of the flight from London to Tangier in one day. No man has done more to educate the public and bring home to them the joys and advantages of flying and no man has written quite such an interesting record of his experiences on the continents of the world. Altogether a book thoroughly to be recommended.